# CRITICAL NOTICE AND APPRECIATION OF THE WORK OF THE LATE PROFESSOR CLARK L. HULL

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*A Behavior System.* BY CLARK L. HULL. Yale University Press. 1953. (London: Geoffrey Cumberlege. English price 40*s.* net.)

The peculiar difficulties of explanation in Psychology are indicated in the many and diverse systems which have appeared from time to time and particularly in the present century. Nearly all of them have enhanced our understanding of at least some facets of behaviour and some have proved more susceptible of extension and application than others. It could be claimed that the Behaviouristic method did not begin with J. B. Watson's forthright pronouncements; but despite apparent departures from or weaknesses in planning and consequent inconsistencies in terminology, Watson had suggested a broad plan which others were to follow and the most notable of these, as builders of systems, have been Prof. E. C. Tolman and the late Prof. Clark L. Hull. Tolman's *Purposive Behavior in Animals and Men* (1932) presented an ingenious system based upon the two assumptions that (*a*) 'Behaviour, as behaviour, that is, as molar, is purposive and is cognitive. These purposes and cognitions are its immediate descriptive "warp and woof"', and (*b*) that, 'such purposes and cognitions are just as evident, as we shall see later, if this behaviour be that of a rat as if it be that of a human being'. Tolman's attribution to the environment of features of a kind which are no doubt significant for the animals, like Lewin's allocation of degrees of significance reflecting the condition of the organism, gave an interesting review of the possibilities within a particular behavioural situation at a given time; but as Lewin himself conceded, such allocation presumed that all the problems of Psychology had been solved, and it is impracticable, except in repeated trials, to determine with any precision the several 'valences', 'topological stresses', 'discriminanda', 'manipulanda' and 'manipulanda expectations' and the relationships between the kaleidoscopic changes from one moment to another.

Hull's intention was to begin in a different way. As he wrote (1943*a*), 'an ideally adequate theory even of so-called purposive behaviour ought, therefore, to begin with colourless movement and mere receptor impulses as such, and from these build up step by step both adaptive behaviour and maladaptive behaviour. The present approach does not deny the molar reality of purposive acts (as opposed to movement), of intelligence, of insight, of goals, of intents, of striving, or of value; on the contrary, we insist upon the genuineness of these forms of behaviour. We hope ultimately to show the logical right to the use of such concepts by deducing them as secondary principles from more elementary objective primary principles'. Throughout the development of such a system, the criteria of explanation were to be that, 'an observed event is said to be explained when the proposition expressing it has been logically derived from a set of definitions and postulates coupled with observed conditions antecedent to the event' (1943*b*). The science of behaviour was regarded as 'a true, though immature natural science'. (Personal communication, 20 Oct. 1949.)

In many ways the *Principles of Behavior* (1943) offered an integration of the treatment of facets of behaviour which had appeared in journals since the late 'twenties. Notable among these were: 'Knowledge and purpose as habit mechanisms' (1930), 'The goal-gradient hypothesis and maze learning' (1932), 'The mechanism of the assembly of behaviour segments in novel combinations suitable for problem solution' (1935) and 'Mind, mechanism and adaptive behaviour' (1937), the latter being an account of a miniature system in which aspects of purposive behaviour were deduced in thirteen theorems from a number of simple postulates. The interesting feature in these papers was that a behaviouristic explanation was attempted for 'insight', 'antedating reactions' and some types of problem-solving, phenomena hitherto 'explained' by the mention of the term 'insight'—and the process of understanding for the reader began with simple empirical findings associated with conditioning.

It is obvious that the extent of such systems, as represented by the number of postulates and definitions, the deductions therefrom and possible applications is limited by our knowledge of the functions of nervous systems and reliable observations of behaviour; but the *Principles of Behavior* did present the most rigorous and extensive behaviouristic system to date and clearly involved considerable erudition and painstaking care in collating a large number of empirical findings. There were sixteen postulates and a number of corollaries which dealt with several factors relevant to the adjustment of the organism to its environment. These included Reinforcement, Stimulus Generalization, Motivation, Inhibition, Oscillation and Reaction Latency. The basic concept which provided the instigation for the system was that of need, which was claimed to exist 'when a condition arises for which action on the part of the organism is a pre-requisite to the optimum probability of survival of either the individual or the species' (1943*c*). The need was claimed to produce the primary animal drives. One could ask whether or not Hull had contradicted his plan to refrain from introducing such concepts as purposive action, insight, goals, intents and striving until they could be deduced 'as secondary principles from the more elementary objective primary principles'. The impression that many of the quantitative examples were artificially simplified and that in the absence of conclusive data or indications as to method, the model of the physical sciences was invariably imposed, did arise; but detailed queries and points of disagreement should not be allowed to detract from the achievement as a whole. Hull had drawn attention to the possible inter-relation of a number of concepts involved in adaptive behaviour. The general plan admitted of extensions and refinements (which Hull and his co-workers pursued assiduously and published frequently in cyclostyled form) and of the behaviour covered by the system, the reader did feel that he had gained an understanding not hitherto available to him.

The continued work of refinement found expression in the *Essentials of Behavior* published in 1951. Once more the basic concept was need reduction and the eighteen postulates described again the fundamental processes by which behavioural adjustments could be made and further adaptations deduced. The order of the postulates is slightly different from that in the *Principles of Behavior* and the wording and mathematical expression has in several places been revised. A criticism which was anticipated in the *Principles of Behavior*, and discussed elsewhere by Hull (1945), is met directly by the last postulate—No. XVIII—'The "constant" numerical values appearing in equations representing primary molar behavioural laws vary from species to species, from individual

to individual, and from some physiological states to others in the same individual at different times, all quite apart from the factor of behavioural oscillation ($_{S}O_{R}$)'.

In a personal communication dated 20 October 1949, Hull mentioned a further work 'on individual, as distinguished from social, behaviour' or as he describes it in the preface to *A Behavior System* published in 1952, 'intended primarily to show the application of the *principles*[1] to the deduction of the simpler phenomena characterizing the behaviour of single organisms'. There was too the plan for yet another work which 'would apply these same principles—to the deduction of the elementary phenomena of social behaviour, i.e. of behaviour manifested when the interacting objects are mammalian organisms of the same species'; but in the same preface Hull states, 'I greatly regret that in all probability I shall not be able to write the third volume'. In his letter of 20 October 1949, Hull indicated that he 'was very ill with coronary thrombosis....This condition combined with the fact that I am sixty-five years of age, greatly limits my capacity for work.... If I live to the age of retirement (68 at Yale) I ought to finish the volume....' As Prof. Mark A. May points out in a Foreword to *A Behavior System*, 'Hull won the race'. He died on 10 May 1952, after the MS. had gone to press, and did not see the galley proofs.

The book is admirably arranged and printed. There are eighteen postulates and fifteen corollaries in *A Behavior System*, following broadly the same lines as in the *Essentials of Behavior*. These are set out at the beginning with the mathematical equations for the postulates and their subsections numbered consecutively, giving to date, thirty-one basic equations, many of which embody provisional numerical approximations. The postulates may be regarded as the most defensible form of statement available to the author in regard to a particular aspect of behaviour, such as the constitution of Reaction Potential, Inhibitory Potential or Stimulus Generalization. They are used in the latest work more as summarizing a general background of knowledge upon which the processes of reasoning in the 133 theorems are based. Theorem 1 deals with the course of the development of two super-threshold, reaction potentials in competition. Theorem 30 deals with the results of shifts in incentives as suggested by the work of Crespi (1942)—'Other things constant, an abrupt shift in the incentive used during a maze-learning process will be followed first by a major shift in reaction potential and then by two or more progressively smaller shifts on successive trials, the series constituting a rapid learning process of the exponential variety, culminating in the course that $_{S}E_{R}$ would have followed had the new incentive been operating continuously from the beginning of the learning'. Theorem 48 states, 'other things equal, the repeated performance and reinforcement of simple behaviour links tend to shift the contraction-intensity phases of the several muscles in such a way as to attain the maximum of reinforcement which is consistent with a minimum of work'. Theorem 96 states: 'When an indirect member of a locomotor habit-family hierarchy has attained a goal in a novel situation involving relatively free space, the learning then acquired is transferred to the initial segments of remaining members of the hierarchy without specific practice and on subsequent trials is manifested by the organism's spontaneous choice of the most direct path'.

Perhaps the most interesting extension of Hull's approach for those working in the social sciences is the clearly defensible attempt in chapter eleven to treat the concept of value and valuation behaviouristically. The treatment begins with a consideration of nutritional needs which have an interesting and methodologically convenient evolutionary

[1] Italics mine.

continuity, and while the scheme is of necessity not worked out in detail, enough is indicated to suggest that an extension to other activities is possible.

In a personal communication dated 29 February 1952, Hull, with characteristic modesty, wrote, 'when one is in the midst of system making the great gaps in the structure are so evident that the task is very discouraging'. Few realized more clearly than Hull the limitations of his systems. Of *A Behavior System*, he could write in the same letter, 'More than ever I realize the inadequacy of the work—the parts I have left out. Really any system written at present must be a miniature one in comparison with the true system as it will some day be known'; but the estimate of Hull as an architect of systems cannot be influenced by considerations of extent alone, nor the very provisional numerical coefficients and the artificially simplified examples. He was intent upon indicating lines of development and was ever ready to revise when further information came to hand, regarding agreement of the deductions made with empirical fact as the criterion of the soundness of the system. To arrive at the same result by two entirely different routes, that is by a process of deduction from basic axioms or postulates and by a process of generalization from empirical findings is certainly an effective method of allaying the provocation of a human mind intent upon explanation and, despite the inevitable limitations, in his time, Hull's systems do leave the student with the impression of penetration and the prospect of further extension as and when reliable behaviouristic designations of relevant states become possible. It is of interest to note that 52% of the 178 theoretical propositions in his latest work were thought to have empirical confirmation and a further 17% to have approximate or indirect evidence in support.

It would be easier in 1970 to assess Hull's influence upon the course of Psychology; but without undue hazard, attention could be drawn to certain features in 1954. There is the definite stress on objectivity and experimental method, apparent in his *Aptitude Testing* (1928, 1929) and *Hypnosis and Suggestibility* (1933) which marked a departure from much of the writing in the earlier years of this century when reference to experimental test lagged far behind the invention of plausible and attractive hypothetical systems. There is the improved orientation in the fields of general behaviour and learning which derives from his efforts in collating the experimental evidence relating to reinforcement, stimulus generalization, inhibition, oscillation and reaction latency, the consequent stimulus to investigation in these fields and, perhaps most important of all, there is the acceptance of the challenge that many of the questions addressed to Psychology are in the form of those addressed to a natural science, indicating the desire to know how the particular behavioural events take place. In response to this challenge there are the works, *Mathematico-deductive Theory of Rote Learning* (Hull, Hovland, Ross, Hall, Perkins & Fitch, 1940), *Principles of Behavior*, *Essentials of Behavior* and *A Behavior System*, in his own view as yet sadly incomplete as systems of explanation but, in principle, capable of extension not only in the fields in which he and his closer colleagues were engaged, but to such topics as attention, see Berlyne (1951), the vexed literature of emotions, see Brown & Farber (1951) and in J. G. Taylor's forthcoming work on Perception.

In conclusion, Hull's personal achievement, in bringing to a definite point his contribution to the efforts of the Institute of Human Relations at Yale to develop a basic science of behaviour, deserves special mention. The foregoing is essentially an academic appreciation of Hull, since we never met. I had hoped to meet him at the International Congress

of Psychology in Edinburgh in 1948; but he was too ill to come. It is more appropriate that the appreciation of his personal qualities should be made by those who worked with him for many years; but one feels bound to acknowledge a prompt and generous correspondence, always marked by scientific humility and appreciation of help from his colleagues.

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# THE EFFECT OF THE VOCATIONAL AIMS OF INDUSTRIAL APPRENTICES UPON THEIR ATTITUDE TO EDUCATION AND COMMISSIONING[1]

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## I. Introduction

The aim of this paper is to show that the vocational aspirations of a large section of the population of the United Kingdom lead them to reject opportunities of filling roles associated with superior social status. First, this section of the population will be defined; secondly, I shall attempt to show that many included in this group have opportunities for filling superior status roles; and thirdly, that they tend to reject such opportunities. Lastly, the motives of the behaviour described will be discussed.

## II. The population

The population studied consists of a sample of industrial apprentices; men who spend five years between ages 16 and 21 learning a skilled trade. Apprentices enter the R.A.F. from about 400 skilled trades (that is to say, the names of distinctive occupations listed for apprentices entering over a period of $1\frac{1}{2}$ years totalled about 400), the range and quality of which may be briefly illustrated by quoting twenty from an alphabetically ordered list: *Radio mechanic, radiographer, railway engineer, reed maker, reel maker, refrigerator mechanic, rivetter, rockman, roll turner, ropemaker, rubber worker, saddler, sailmaker, sanitary engineer, saw doctor, sawsmith, sawyer, scale fitter, scientific instrument maker, scythe grinder.*

The training given and the skill acquired by the apprentice vary greatly in different trades, and even within trades between different areas and employers. These sources of difference do not now concern us. The educational level of apprentices also varies greatly. Many an apprentice continues his formal education regularly at evening classes and perhaps also at day continuation school, taking examinations from the preliminary level through the national engineering series, sometimes up to B.Sc. Others discontinue formal education a year or two before beginning apprenticeship and may continue to neglect it. The range of educational achievement descends to the borderline of illiteracy.

Apprentices form about one-quarter of the whole population of young men accepted for national military service annually in the United Kingdom, as will be seen from the analysis in Table 1.

The majority of national service recruits are called up at about 18 years of age, but apprentices usually at the age of 21. In a representative sample of 2% of all national service recruits entering the R.A.F. in the year following 1 August 1951, 66% entered

[1] Paper delivered at a meeting of the British Association for the Advancement of Science, Liverpool, September 1953. Published by permission of the Air Ministry.

at the age of 18, about 8% after deferment for University or other professional studies, and about 25% after deferment for completion of apprenticeship. This sample of apprentices numbered 224 men, and represented over 11,000 apprentices entering the R.A.F. during the year.

## III. METHOD

The grammar school is an avenue to superior social status. The Queen's commission confers such status. To show that industrial apprentices tend to reject opportunities for filling superior status roles, it must be shown (i) that a certain proportion of them fulfil the elementary qualifications for admission, (ii) that a proportion of those so qualified reject such opportunities, and (iii) that the proportion so behaving is significantly greater than that in a group representative of a non-apprentice population under comparable conditions. The method employed was (*a*) to compare the scores on a test of general intelligence and the schooling of the apprentices with those of the main body of the 2% recruit sample,

Table 1. *Disposal of men liable for military service under the National Service Act* (1948)

(Average numbers for age classes of 1933–1934 inclusive.)

| Disposal | Registered | | Accepted |
|---|---|---|---|
| | No. | % | (%) |
| Posted to H.M. Forces (as N.S. entrants or as volunteers) | 144,700 | 51·5 | 66·2 |
| Deferring | | | |
| (*a*) for apprenticeships | 53,500 | 19·1 | 24·5 |
| (*b*) for University or professional studies | 20,400 | 7·3 | 9·3 |
| Total acceptable | 218,000 | 77·9 | 100·0 |
| Exempt owing to occupation | 22,900 | 8·1 | — |
| Unfit | 36,900 | 13·2 | — |
| Others | 2,100 | 0·8 | — |
| Total registering | 280,500 | 100·0 | — |

Entries calculated from statistics published in the *Ministry of Labour Gazette*, May 1953.

taking into account also the statements of individuals on their educational experience in relation to their vocational aims, and (*b*) to compare the proportion of applications for commissions made by apprentices with those of other national service recruits under similar conditions, using another sample comprising only recruits eligible to apply for commissions.

The two opportunities studied do not occur entirely independently. The preliminary screening of national service candidates for commissions is by an educational qualification (formerly the possession of a School Certificate or a certificate of equivalent standing). Of the total national service recruit sample studied, 37·5% qualified in this way. Of these qualified men, 80% had attended grammar or public schools and 14% technical secondary schools full time. Entry to technical schools is frequently determined mainly by results on the examination which governs grammar school entry.

## IV. REJECTION OF EDUCATIONAL OPPORTUNITY

On entry every recruit is given certain tests of general intelligence and aptitude. He also fills in a form giving particulars of his education, including the school or college last attended full time. Details are later amplified and checked in individual interviews. At the interview, some men who have entered *secondary modern* for their last full-time schooling,

refer to the examination at the age of 11+ and say that they voluntarily did not sit, or that they had to enter but did not wish to succeed in securing grammar school education.

These verbal responses, recorded by the interviewers, have not been systematically studied because the interviews are not rigidly standardized, and current report of attitudes held seven to ten years previously is not reliable evidence of the motives of behaviour at that time. But other evidence of the relation between educational background, abilities and apprenticeship of recruits suggests that such statements may represent not a sour grapes attitude nor any form of defence against a sense of inferiority, but a constant negative reaction to opportunities for grammar school education. The evidence is presented in tables of very simple form. The schooling statistics conceal regional and the mean test scores individual variation, which have both, however, been studied in detail. Analyses of variance support the argument presented by simple comparison of means and percentages.

Table 2. *Apprenticeship and post-primary education*

| | National service recruits to the R.A.F. | | | |
|---|---|---|---|---|
| | Apprentices | Others | Total sample | |
| Post-primary education | | | % | No. |
| Secondary modern | 57 | 40 | 45 | 376 |
| Grammar, public or technical full time | 43 | 60 | 55 | 466 |
| Total, % | 100 | 100 | 100 | — |
| Total, No. | 224 | 618 | — | 842 |

Table 3. *Apprenticeship and abilities*

| | Mean scores | | | |
|---|---|---|---|---|
| R.A.F. test | Apprentices | Others | Total | Score range |
| General intelligence | 37·46 | 37·74 | 37·67 | 0–60 |
| Verbal comprehension | 51·24 | 51·49 | 51·42 | 0–80 |

It will be seen from Table 2 that the apprentice entry includes a significantly larger percentage of men whose last full-time schooling was secondary modern or equivalent. (By $\chi^2$ test of significance of difference, $P < 0{\cdot}001$.) Yet the average score made by these apprentices on two paper-pencil tests of ability is equal to that made by other recruits. The fractional differences recorded in Table 3 are not statistically significant, by $t$ test.

It is suggested that the divergence in schooling between the two groups arose because the apprentice group included boys whose intelligence was associated, level for level, with less wish to succeed in the 11+ examination for grammar school places. The findings would also be consistent with the exclusion of only very dull boys from apprenticeship, in an otherwise representative sample. The latter alternative may be dismissed, since apprentice scores range as low as those of the other group and the total variance of scores of the two groups shows no significant difference (by $\chi^2$, $P$ approximates 0·3).

The R.A.F. tests aim to measure general intelligence and verbal comprehension. The 11+ examination for entry to grammar school aimed at the same sort of thing with the same men some years earlier. Table 4 offers some evidence that the two procedures were assessing similar or closely associated attributes. The average score of the secondary modern old-boy on R.A.F. tests is significantly lower than that of the boy who succeeded in the 11+ examination. The findings are suggestive, not conclusive, because in these data

the effect of different secondary schooling upon R.A.F. test results is not separately measurable. By $t$ test, $P < 0{\cdot}001$ that the differences between column entries in Table 4 would occur by the chances of random selection.

It appears then that the apprentice group is on average as intelligent as the rest of the body of R.A.F. recruits, but its success in the grammar school entry examinations is less than average, which may be because in many cases success would have conflicted with the aim of training to be a highly skilled tradesman.

## V. REJECTION OF COMMISSIONING OPPORTUNITY

These decisions about schooling were made years back; the decisions about commissioning are being made to-day. Every Air Force national service recruit with the required educational qualifications is encouraged to apply for a commission for the period of his national service. The number of commissions granted is small compared with the number

Table 4. *Post-primary education and abilities*

(2% sample of national service recruits to the R.A.F. over a year.)

| | R.A.F. test mean scores | | |
|---|---|---|---|
| Post-primary education | General intelligence | Verbal comprehension | No. of men |
| Secondary modern | 33·24 | 44·35 | 376 |
| Grammar, public or technical (full time) | 41·23 | 56·89 | 466 |
| Total sample | 37·67 | 51·42 | 842 |
| Test score range | 0–60 | 0–80 | — |

Table 5. *Apprenticeship and commissioning applications*

| | Recruits educationally eligible | | Total | |
|---|---|---|---|---|
| | Apprentices (%) | Others (%) | (%) | No. |
| Apply for commissions | 67 | 81 | 78 | 870 |
| Do not apply | 33 | 19 | 22 | 243 |
| Total, % | 100 | 100 | 100 | — |
| Total, No. | — | — | — | 1113 |

of men qualified to apply, and may vary periodically according to manning requirements. But within any one period the selectors' aim is to keep the standard constant.

Those educationally qualified are called Potential Officer Material—P.O.M. Out of over 11,000 apprentices entering during the year, over 2500 were P.O.M. These men go to a camp together with the other P.O.M. for basic recruit training, and while there they are all invited to apply for commissions, interviewed by selecting officers, and if considered suitable, sent forward for the final decision of an Air Ministry Selection Board. (In addition to the national service P.O.M. from this camp, the Board also interviews a number of University graduates before their call up, who, if accepted, proceed direct to officer cadet training or to a medical officers' unit. Consequently the figures tabulated are not a complete account of the commissioning of national service recruits.)

Using the documentary returns from the P.O.M. camp over a period of four consecutive weeks, we may compare the percentage of apprentices and of non-apprentices who applied for commissions, and the percentage of these applicants who were passed by the preliminary and final selectors. It is clear from Table 5 that the percentage of apprentices applying for commissions was much lower than that of the other eligible recruits. The

difference between the column entries is highly significant statistically ($P < 0{\cdot}001$, by $\chi^2$ test).

Clearly the apprentices have imposed upon themselves a severe self-screening process by which, on average, one in every three men eligible has decided *not* to apply for a commission. The self-screening of the other recruits is less severe—four out of five apply.

Following the applicants through, we find that selection on the station gives the results shown in Table 6.

Again the differences between apprentices and others is significantly greater than would be likely to arise by chances of sampling (by $\chi^2$ test, $P < 0{\cdot}001$). The selectors on the camp send forward a higher proportion of the apprentice applicants, 35% as against 11%, and so tend to correct the men's own severe self-screening. This finding was unexpected both by the researcher and by the selectors. The selectors were anxious that all eligible recruits should volunteer for commissioning and be given equal consideration, but they had not looked upon the apprentices as better candidates than the others (which,

Table 6. *Apprenticeship and commissioning: provisional selection*

| | P.O.M. applicants for commissions | | | |
|---|---|---|---|---|
| | Apprentices (%) | Others (%) | Total over 4-week period | |
| | | | (%) | No. |
| Recommended | 35 | 11 | 15 | 131 |
| Rejected | 65 | 89 | 85 | 739 |
| Total applying, % | 100 | 100 | 100 | — |
| Total applying, No. | — | — | — | 870 |

Table 7. *Apprenticeship and commissioning: final selection for O.C.T.U.*

| | Recommended by provisional selectors | | | |
|---|---|---|---|---|
| Final selection for training | Apprentices (%) | Others (%) | Total | |
| | | | (%) | No. |
| Selected | 23 | 29 | 27 | 35 |
| Rejected | 77 | 71 | 73 | 96 |
| Total recommended, % | 100 | 100 | 100 | — |
| Total recommended, No. | — | — | — | 131 |

counting all those eligible, they may not have been), and had therefore not considered the apprentice *applicants* as better candidates than the others. But the selectors' actual assessments of individual cases had remained independent of their general judgements.

In the final selection for officer training, as shown in Table 7, the percentage of apprentices recommended by the station selecting officers and passed by the Board is somewhat lower than that of the other recruits, but the difference is statistically insignificant. That is, the Air Ministry Selection Board tends to pass in about the same proportion the apprentices and the other recruits sent forward by the selection officers who undertake the preliminary selection screening on the camp.

This concludes the evidence presented to establish the proposition that industrial apprentices tend to care less than other members of the population about acquiring officer rank and grammar school education. But it tells little about the impulses behind this behaviour. An attempt is in progress to find out more, by interviews and questionnaires given to apprentices on the P.O.M. training unit, and although the results are not complete, they give rise to suggestions that may be of use in interpreting the total material.

## VI. DISCUSSION

We appear to be concerned, then, with two forms of social impulse, not primary, but operating in adults with some independence; a wish to acquire and practice an occupational art or skill, and a wish to acquire or maintain high social status. These impulses may act in parallel, and questions of their relative strength then tend to be overlooked. Alternatively, in certain circumstances, they may co-exist at cross-purposes. The way a man acts will then tend to be related to the degree of divergence and the relative strength of these impulses, and his opportunities for realizing them in action.

Two groups have been studied, in two fields of action: the apprentices and the remaining body of recruits, as applicants for secondary school places and for service commissions. In both fields it is generally agreed that considerations of social status operate as an incentive to achievement. In both the apprentice group achieves less than its abilities warrant. The vocational aims of the apprentices are unusually strong and clearly defined. They are not directed towards roles bearing high social status. It appears probable that the observed short fall in achievement is largely due to the divergence of the two aims and the greater strength of the vocational impulse.

Whether the desire for social status is weaker among the apprentices than among the general body of recruits does not appear. There is some evidence in the more recent data that temporary frustration of the vocational impulse permits a desire for social status to operate as strongly in the apprentice as in the general group and perhaps often more naïvely. In the officer selection process the total apprentice group tends to divide into two, one including those who have a hope of getting their own trade in the Air Force, and the other, those who have not. The attitude to officer rank of what we may call the trade-frustrated group, is different from that of the apprentice group in general. The trade-frustrated group rate the opportunity to be commissioned more highly.

Finally, a brief reference to social values. Because desire for higher social status frequently acts in line with vocational impulse, the distinction is apt to be overlooked. It may even be suggested that the two impulses *ought* to act in parallel—every man *should* desire grammar school education and commissioned rank. Actually the social health of the community may be positively associated with the strength of vocational impulses rather than with desires for higher social status. It would follow that the attitudes of the apprentice-artisan group, as they have been described, are not to be disapproved or discouraged.

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# THE EFFECT OF REFERENCE GROUP FUNCTIONS ON SOCIAL STATUS RATINGS[1]

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I. *Introduction* (p. 88). II. *Method and source of data* (pp. 88–89). III. *Hypotheses formed on initial data* (pp. 89–90). IV. *Method of checking hypothesis* (p. 90). V. *Results obtained* (pp. 90–92). VI. *Summary* (pp. 92–93). *References* (p. 93).

## I. Introduction

This paper is designed to show how a person's rating of the status of his social role can be affected by the satisfaction he gets from that role. For example, if being an Air Force sergeant is a very satisfying and important thing in a man's life, then when he is asked to rate the social status of the role of Air Force sergeant, he will tend to rate it higher than most people would. In the same way a man from a lorry driver's family who has become an Air Force officer will probably want to reject his association with his social background and will therefore rate its status lower than would most other people for whom it has no such negative associations. A group to which a person belongs and membership of which has some such marked value for him I shall call a 'reference group' (Hyman, 1942; Newcomb, 1943). This is only one usage of the term reference group which, in other usages, may not be restricted to membership groups (Kelley, 1952). The material to be drawn on is taken from a larger study which was not designed to demonstrate this hypothesis, so support for it can be given in general terms only and no detailed study of the social and psychological processes involved is attempted.

## II. Method and source of data

The work from which the material was taken was concerned with the social mobility experienced by members of the R.A.F. in moving from civilian life to Air Force life. The two points of reference used were the social status of a man's family as represented by his father's occupation and the social status of his Air Force role. From each man a subjective and an objective rating of the social status of both these roles was obtained. Both types of rating were made in terms of seven typical occupations forming a standard seven-point scale adapted from the one set up by Hall & Jones (1950). The subjective ratings were the man's own assessments of the positions on the standard scale at which the two roles belonged. The objective ratings of family backgrounds were made on the basis of the Hall-Jones criteria, while the objective ratings of Air Force roles were arrived at from this research.

The seven-point scale of occupations used as a standard ranged from professional to unskilled manual occupations through the following groupings. The example of each given in parentheses is the example used in the research. Group one, professional and

[1] Paper delivered at a meeting of the British Association for the Advancement of Science, Liverpool September 1953. Published by permission of the Air Ministry.

higher administrative (company director); group two, managerial and executive (works manager); group three, higher grade inspectorial and supervisory (elementary school teacher); group four, lower grade inspectorial and supervisory (head clerk); group five, routine non-manual and skilled manual (clerk, carpenter);[1] group six, semi-skilled manual (lorry driver); and group seven unskilled manual (dock labourer).

The sample used was a group of 194 R.A.F. personnel made up of random samples of airmen (both regular and National Service airmen), non-commissioned officers, and officers. It was found that the mean rating given the standard occupations by the men in all these groups was the same for each of the occupations forming the standard scale as these occupations were given in the original Hall-Jones Study. From this evidence of the reliability of the use of the standard scale by the sample, it was assumed that the mean ratings given to the Service roles by the whole sample could be taken as a sufficiently accurate assessment of their social status in terms of the standard scale. These ratings were therefore taken as the objective status ratings of the Air Force roles. There were four roles involved—Regular Airman, National Service airman, R.A.F. Senior N.C.O. and R.A.F. Officer, one or other of which of course applied to every man in the sample.

With these four service ratings it was possible to construct an objective and a subjective mobility rating for every man. These mobility ratings were both expressed in terms of distance along the standard scale. The objective mobility rating was the number of steps between the objective status ratings of a man's family and of his R.A.F. role. The subjective rating was the scale distance between the man's own assessments of these two roles.

## III. Hypotheses formed on initial data

Comparison of objective and subjective mobility ratings disclosed that in many cases these were different. Many of the minor differences could be accounted for from the nature of the rating technique which had been used. In using the technique it was possible for subjects to make a rating between points on the standard scale as well as on a level with them. However, even when minor differences arising from this cause had been allowed for, there was still quite a number of men who had either overestimated or underestimated their social mobility in joining the Air Force. It was, of course, possible that these discrepancies could have arisen from inability to rate occupations and R.A.F. roles accurately. In rating the standard scale, however, it had been found that all groups in the sample had used the status rating scale with considerable accuracy. It was therefore unlikely that inability to use the scale, or a failure to understand it, would explain the discrepancies observed in the assessments of mobility.

An hypothesis was developed to explain this deviant status rating behaviour in terms of reference group functions. It was suggested that if a man's R.A.F. role or his civilian social status had a strong reference group association for him, he would tend to rate such a group abnormally because of this. This supposition was based on the fact that ratings of social status as well as being concerned with recognizable facts about the prestige differences of social groups, also tend to be value judgements. In general, high status is considered to be more desirable than low status. Consequently, when rating a group with which one likes to be identified (that is, a positive reference group), there is a tendency

[1] Group five was originally divided into 5A (routine non-manual), and 5B (skilled manual). But as only 2% of the sample fell in group 5A, it was combined with 5B as one group.

to raise its status, Consequently, when rating a group of which one is a member, but with which one does not like to be identified (that is, a negative reference group) one tends to lower its status. Now the present hypothesis is that many men for whom their R.A.F. roles represent a positive reference group will tend to make abnormal ratings of their social mobility in joining the R.A.F. because they will overrate their civilian status. As a corollary to this it would also be expected that men who have adopted an R.A.F. reference group might depress their native civilian status group because (especially for the upwardly mobile) it may have acquired a negative reference for them. Similarly, men whose principal reference group is civilian might underrate their R.A.F. group as one with which they have no particular desire to be associated.

## IV. Method of checking hypothesis

In order to analyse the ratings of the men in the sample along these lines, the men were divided into three groups, according to the type of mobility ratings they made. These groups are labelled positive mobility raters, neutral mobility raters and negative mobility raters. The positive mobility raters were either men who had been upwardly mobile in joining the R.A.F., and who overrated the extent of this upward mobility, or men who had been downwardly mobile in joining the R.A.F. and who had underrated this downward mobility. The neutral mobility raters were those whose assessment of their mobility agreed with or was not greatly different from an objective rating of it. The negative mobility raters were either men who had been upwardly mobile and who underrated this upward mobility, or men who had been downwardly mobile and who overrated this downward mobility.

If the hypothesis just outlined is to be substantiated, it is necessary to find some evidence that the positive mobility raters take an R.A.F. role as a positive reference group significantly more frequently than do the negative mobility raters. And on the other hand, evidence is needed to show that the negative mobility raters look to a civilian reference group more often than the positive mobility raters. As has already been said, this small study is a by-product of a larger research which was not designed to test this particular hypothesis. The only direct evidence is in the status rating behaviour of the two groups, and because of small numbers this cannot be analysed in any great detail. There are, however, several lines of indirect evidence.

## V. Results obtained

From the research a rating was available for each subject as to the relative satisfaction or dissatisfaction he felt towards his life in the R.A.F. In Table 1 the reactions of the two groups who made abnormal ratings of their civilian to service mobility are shown.

From Table 1 it can be seen that the positive mobility raters are significantly more often satisfied with R.A.F. life than are the negative mobility raters, while the latter group contains a significantly greater proportion of men who express dissatisfaction with R.A.F. life. These differences are in the direction predicted by our hypothesis and so provide some support for it.

If the process indicated in our hypothesis were the only reason why men tend to assess their mobility incorrectly we would expect the differences in the degree of satisfaction

expressed by the two groups to be rather larger than they are. There are probably two reasons for this. First, in fact our hypothesis is not the only explanation of the tendency to make abnormal ratings of mobility, and secondly, the reason might lie partly in the shortcomings of the particular technique used in this instance to record degree of satisfaction with R.A.F. life. We will return to the first point later. To consider the second point, the form of the technique from which service satisfaction was assessed led subjects to make their judgements on an objective basis rather than a R.A.F. basis. They were invited to consider the satisfaction they found in their R.A.F. roles against a background of all roles of all men rather than only of men in the R.A.F. It is this that has probably caused the bunching at the 'satisfaction' end of the scale and so a poor scatter.

Two other lines of supporting evidence have to do with the rank composition of the two groups. It would seem reasonable to assume that officers and senior N.C.O.'s in the R.A.F. would tend to be more identified with the service than airmen. Officers and senior N.C.O.'s are older men who have decided to make a career of the R.A.F. and have had the reward of acceptance in a higher prestige group to confirm them in this decision. Airmen, on the other hand, are younger and are much less confirmed in a service career. Many of them will leave the service while still quite young men. Also they do not have positions of responsibility in the functioning of the service structure as the more senior men have, and are so less likely to feel closely identified with the service. It would seem safe therefore to assume that, on the whole, officers and senior N.C.O.'s are more likely

Table 1. *Relation of type of mobility rating to degree of satisfaction with R.A.F. life*

| Degree of satisfaction with R.A.F. life | Type of mobility rating: Positive (%) | | Negative (%) |
|---|---|---|---|
| Satisfied | 71 | —S— | 52 |
| Neutral | 24 | | 31 |
| Dissatisfied | 5 | —S— | 17 |
| N= | 100% | | 100% |
| | 38 | | 72 |

—S— = statistically significant difference $P = 0{\cdot}05$.
'Positive mobility rating' = exaggerated upward or underestimated downward mobility.
'Negative mobility rating' = exaggerated downward or underestimated upward mobility.

to take their R.A.F. group as a positive reference than are airmen. Thus, it would be expected from our hypothesis that the positive mobility raters would tend to contain more officers and senior N.C.O.'s than the negative raters. And this is in fact found. Among the positive raters 68% were officers or senior N.C.O.'s, and among the negative raters 47%. This difference is statistically significant.

Another argument along the same lines has to do with the social status rating the two groups gave to the role of R.A.F. officer. It might be expected that men who feel closely identified with an institution would tend to rate the social status of the senior role in the institution more highly than would men less identified with it. The social status of such a role is in part a symbolic rating of the institution as a whole. In our two mobility groups 70% of the positive raters placed the role of R.A.F. officer in one of the highest two social status groups, while 50% of the negative raters did so. This difference is statistically significant and is in the direction to be expected from our hypothesis.

Finally, there is the analysis of the manner in which the two groups produced the abnormal ratings of their mobility. This is set out in Table 2.

There are several points of interest in this analysis. Consider first the ratings of the positive mobility raters. It can be seen that the abnormal ratings of these men arise mainly because they have rated their R.A.F. roles higher than the standard rating. These men have also underrated their native status significantly more frequently than have the neutral mobility raters. Among the negative mobility raters the opposite trends are present. They have abnormal ratings mainly because they have overrated their native status. They also underrated their R.A.F. roles significantly more frequently than the neutral mobility raters. All these trends are in line with our hypothesis.

## VI. Summary

This paper set out to explain why two groups of men in the R.A.F. who otherwise had shown that they could assess social status differences fairly accurately did not assess accurately their own social mobility in moving from civilian life into the R.A.F. A hypothesis was set up which suggested that the men who exaggerated their upward mobility or underestimated their downward mobility did so because their R.A.F. role had a positive reference for them, while for the men who underestimated their upward mobility or overestimated their downward mobility their more important reference group was civilian. This assumed that when a man assesses the social status of a role which is symbolic (for him) of a positive reference group he would tend to raise its social status above its standard rating. And conversely, he would tend to depress the social status of a negative reference group. Several lines of evidence were considered which supported the suggestion that the men with positive mobility ratings were more identified with the R.A.F. than were the men with negative mobility ratings. Finally, an analysis of the rating behaviours of the two groups showed that the positive raters had a very much greater tendency to raise the status of their R.A.F. roles, while the negative raters showed an equally abnormal tendency to raise the status of their civilian backgrounds. Associated with this there was also a tendency to depress the complementary role, either civilian or service.

Although the patterns of behaviour crucial to the testing of the hypothesis all showed significant trends in the expected directions, they were only trends and not complete conformities. From this it must be concluded that although the hypothesized tendencies

Table 2. *Analysis of abnormal status ratings of men making different types of mobility ratings*

| | Type of mobility rating | | | | | |
|---|---|---|---|---|---|---|
| Deviant status ratings | Positive (%) | | Neutral (%) | | Negative (%) | |
| Own R.A.F. status too high | 60 | —S— | 23 | | 14 | |
| Native status too low | 29 | —S— | 4 | | 3 | |
| Own R.A.F. status too low | 3 | | 6 | —S— | 30 | |
| Native Status too high | 13 | | 12 | —S— | 62 | |
| N = | 38 | | 84 | | 72 | |

—S— = statistically significant difference $P = 0{\cdot}05$.
All differences between corresponding percentages in the two outside columns are statistically significant.
A man may be counted in 2, 1, or 0 rows; thus column totals may be more or less than 100%.

ave been operative, they do not furnish a complete explanation. It is submitted, how-ver, that the material offers a sufficient empirical basis for a strong warning against the ffects of reference group associations when dealing with ratings of social status.

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# ON THE VARIABILITY OF THE SPEED OF TALKING AND ON ITS RELATION TO THE LENGTH OF UTTERANCES IN CONVERSATIONS

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I. *Introduction* (p. 94). II. *The problem* (p. 94). III. *The material* (p. 95). IV. *The method* (p. 95). V. *The results* (pp. 95–104): (i) *Frequency distributions of speech rates* (pp. 95–96); (ii) *Individual differences and situational determination of speed of talking* (*speech rate*) (pp. 97–99); (iii) *The relation of speed and length of utterance* (pp. 99–101); (iv) *Length of utterance and variability in speed of talking* (pp. 101–103); (v) *Length of utterance and mean speed* (pp. 103–104). VI. *Discussion* (pp. 104–106). VII. *Summary* (pp. 106–107). *References* (p. 107).

## I. Introduction

In a previous study of language behaviour (Goldman-Eisler 1954) twelve measurements of the temporal as well as of the grammatical aspects of conversation had been tested by the author for consistency in individuals. The type of conversation, which was a diagnostic interview, was kept constant. Among these measurements, a number of which turned out to be discriminating, the speed of talking (measured as the number of words a minute) was not only the most consistent, but also proved to be the most sensitive aspect of linguistic behaviour.

The measurements of speed of talking in the above experiment, however, were averages of the speed of word output of each individual during a complete interview, i.e. the total word output of the subject was divided by the total time spent talking. The interviews lasted fifteen minutes to half an hour, with an entire word output of 2000–3000 words by each speaker.

## II. The problem

The present paper is concerned with the variability of speed of talking in the course of one speech situation for the different separate utterances. The problems are:

(1) How does the speed of talking of one individual vary within a single conversation for the many separate utterances which he makes in the course of it?

(2) Does speed of talking discriminate between individuals, and what is their variability from conversation to conversation?

(3) What is the relation of speed of utterance to length of utterance? By an utterance or speech action (these two terms will be used interchangeably) is meant a period of speech between the last word of the interlocutor's preceding utterance and the first word of his following one. In the present study speed is measured as the number of syllables a minute, as syllables are more appropriate units than words with which to measure the length of an utterance (Stetson, 1928).

## III. THE MATERIAL

The material for answering the above questions was obtained from ten conversations and group discussions involving four normal adults (three men and one woman), who were all members of the academic and scientific staff of Maudsley Hospital, and four young female neurotics, interviewed each separately on a single occasion by the experimenter. The four normal subjects will be referred to as *A*, *B*, *C*, *D* and the four patients as *L*, *M*, *N*, *O*. Nineteen single records of *A*, *B*, *C*, *D* were derived from the following situations:

*A* talking with *B*, *C*, *D* separately, in a group of three (*A*, *B*, *C*), and in a group of four (*A*, *B*, *C*, *D*).

*B* talking with *A*, *C*, *D* separately, in a group of three (*A*, *B*, *C*), and in a group of four (*A*, *B*, *C*, *D*).

*C* talking with *A*, *B*, *D* separately, in a group of three (*A*, *B*, *C*), and in a group of four (*A*, *B*, *C*, *D*).

*D* talking with *A*, *B*, *C* separately, and in a group of four (*A*, *B*, *C*, *D*).

The material thus consists of twenty-three interaction records. Of these, twelve are records of paired conversations between normals, seven are records of group discussion among normals, while four are interview records between each of the patients and the experimenter.[1]

## IV. THE METHOD

The length of each utterance was timed, while the speech recordings were transferred to paper and the number of syllables counted. The speed of talking for each utterance was then expressed as a rate of the number of syllables a minute. The total number of utterances recorded for *A*, *B*, *C*, *D* (i.e. the normals) was about 1299, ranging between 300 and 400 for each individual, with a mean number for each conversation ranging between 50 and 100 utterances. The length of these utterances varied from one to many hundreds of syllables. For *L*, *M*, *N*, *O* (i.e. the neurotics) there were altogether 376 utterances, with the number of utterances per interview ranging from 78 to 104. Every conversation and group discussion lasted on an average about three-quarters of an hour, and the interviews between half an hour and three-quarters of an hour each.

## V. THE RESULTS

### (i) *Frequency distributions of speech rates*

Frequency distributions were plotted for the talking speeds of each individual separately as shown in Fig. 1. All of these distributions are more or less positively skewed, and are also more or less leptokurtic. There are individual differences in the degree of leptokurticity (peakedness) and in the skewness of the curves representing the talking speeds derived from the individual conversations, but the general description holds for all distributions in the sample in respect of skewness and for six out of the eight distributions in respect of leptokurticity. As an indication of skewness it may be mentioned that the numbers of positive readings beyond +2 S.D. in relation to the negative readings below -2 S.D. are in every case but one a multiple of the latter, the proportion of positive readings surpassing the $2\frac{1}{2}\%$ of total frequencies which one might expect in a normal

[1] Twelve more records of patients in diagnostic interviews with a psychiatrist have since been analysed, confirming our conclusions.

distribution while the number of negative readings below −2 S.D. is considerably less than $2\frac{1}{2}$% of the total frequency. On the average 1% of the total frequency occurred below −2 S.D. and 4·6% above +2 S.D. The peakedness of the distribution is described by comparing the interquartile ranges of the distributions within which the middle

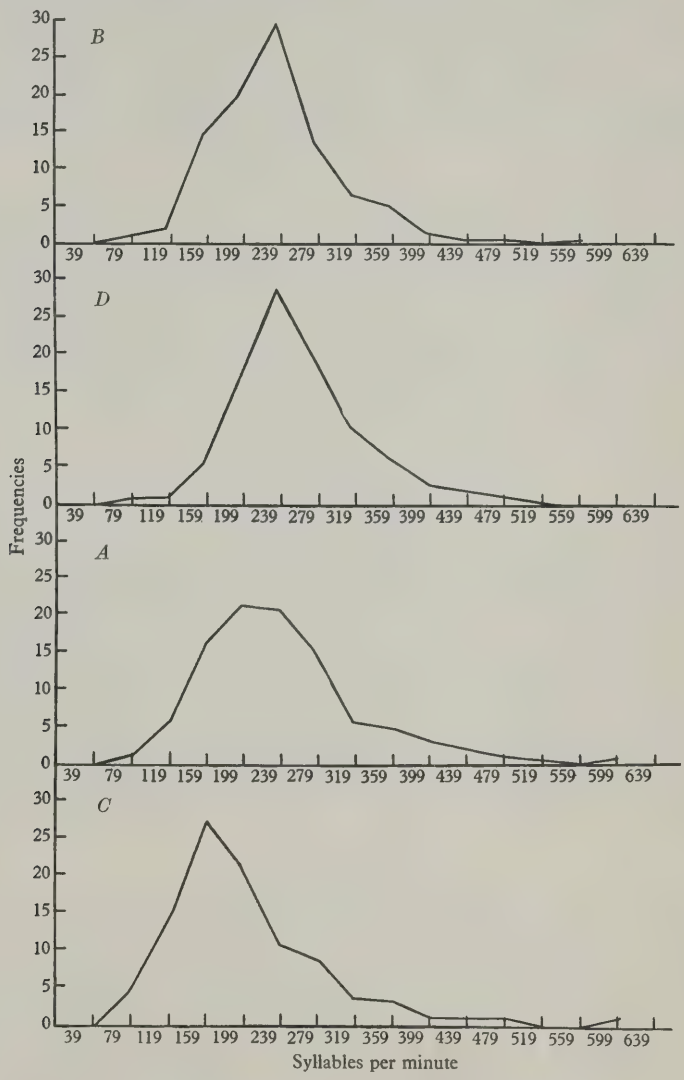



**Fig. 1.** Frequency distributions of speech rates of subjects *A*, *B*, *C*, *D*.

50% of the frequencies lie with the ranges lying between ±0·674 S.D. and ±1 P.E. which would contain 50% of the frequencies if the distribution curves were normal. In six out of the eight sample distributions the middle 50% of the frequencies were concentrated within narrower limits than expected; these curves were clearly leptokurtic as well as skewed. In the case of *C* and *O* the middle 50% lay near enough between the normal limits of ±0·674 S.D. These curves were skewed but normal in respect of peakedness.

(ii) *Individual differences and situational determination of speed of talking (speech rate)*

The subjects *A*, *B*, *C*, *D* and *L*, *M*, *N*, *O* were compared in respect of their speed of talking. Comparisons were made separately for the different lengths of utterance. For *A*, *B*, *C*, *D*, measurements from all their conversational situations were pooled, and in the case of *L*, *M*, *N*, *O*, the measurements were taken from their one interview. Analyses of variance were then computed.[1]

Table 3 illustrates clearly, in terms of means as well as standard deviations, how the speed of talking (speech rate) gradually gains stability as utterances grow longer. Speech rate discriminated between *A*, *B*, *C*, *D* at a high level of significance ($P=0{\cdot}001$) over the whole range of the lengths of utterances from very short to very long ones.

The records of the patients, despite the fact that there were fewer measurements available, showed this differentiation for all but the shortest length of utterance. It is also remarkable that those individuals who were fast talkers in their short utterances remained so in the long ones, notwithstanding the general decrease in speech rate with increase of length of utterance. The rank order of the subjects in respect of speed of talking remained consistently the same over the whole range of lengths of utterance.

Table 1. *Means and standard deviations of speech rates (number of syllables per minute) for subjects A, B, C, D (normals) and L, M, N, O (patients)*

| | Mean* | No. of syllables | S.D. | *P* | | Mean | No. of syllables | S.D. | *P* |
|---|---|---|---|---|---|---|---|---|---|
| *D* | 282·7 | 374 | 84·3 | 0·001 | *M* | 253·5 | 77 | 80·0 | 0·001 |
| *B* | 257·1 | 274 | 77·0 | | *O* | 245·9 | 104 | 68·6 | |
| *A* | 247·3 | 295 | 86·8 | | *L* | 222·1 | 104 | 70·5 | |
| *C* | 208·3 | 357 | 56·2 | | *N* | 214·8 | 78 | 88·9 | |

* The means for *A*, *B*, *C*, *D* are based on readings from all their conversations.

After excluding the short utterances of less than 40 syllables length on account of their wide scatter, measurements of talking speed (speech rate) taken from utterances over 40 syllables long were submitted to an analysis of variance. The mean speech rates in each of the conversations were also compared for each of the four subjects *A*, *B*, *C*, *D* separately. Table 2 lists their means and standard deviations. From it the reader may see that the speed of talking, besides characterizing individuals over and above their fluctuations on different occasions (Table 1) may differ significantly for any one individual when compared for each situation separately.

The nature of the data, being unequal in numbers as well as errors for the groups to be compared, did not favour a three-way analysis of variance sorting out the relative contributions of individuals' speech situations and their interaction, simultaneously. But it was obvious from the separate analyses of variance that all three were significant sources of variation for the speed level of talking in conversations, in proportions varying with the different occasions and individuals. The degrees of variability of individuals from occasion to occasion seems, however, to vary with the individual. Thus *B*'s range of variation is much more flexible than that of *A*, *D*, and particularly *C*. The table also gives an indication of interaction between individual speakers and particular conversations. No systematic effects of the individual interlocutors for all their partners can be observed, although the discussion in which all four subjects took part accelerated all of them.

[1] It is well known that a slight departure from symmetry does not affect the test of significance performed by an analysis of variance very much (Cochran & Cox, 1950).

The complexity of factors determining the speed of talking is illustrated in the following instance. *C*, while having an accelerating effect on *A* in one conversation, did not have this effect in the other, nor when she joined the conversation that had taken place between *A* and *B* alone, as a third in the group. When talking to *B* alone, *A* spoke 216·7 syllables a minute on the average (the total duration was about 30 min.). *C* then joined the group and they continued as a group of three. *A* spoke if anything at a slower rate (209·5) with *C* present. *B*, on the other hand, who had reached his lowest rate in conversation with *C* alone, spoke significantly faster after *C* had joined the group.

Table 2. *Results of analysis of variance testing the differences in speech rates in different conversational situations for subjects A, B, C, D*

| Subject | Conversation | Means | S.D. | *F* ratio | *P* |
|---|---|---|---|---|---|
| *A* | With *B* | 216·7 | 57·5 | | |
| | With *C* | 254·5 | 58·3 | | |
| | With *D* | 206·0 | 80·4 | 4·8 | 0·01 |
| | With *B*, *C* | 209·5 | 23·2 | | |
| | With *B*, *C*, *D* | 244·8 | 24·4 | | |
| *B* | With *A* | 206·1 | 31·3 | | |
| | With *C* | 193·8 | 36·5 | | |
| | With *D* | 252·2 | 56·9 | 16·3 | 0·001 |
| | With *A*, *C* | 237·2 | 52·5 | | |
| | With *A*, *C*, *D* | 275·7 | 42·9 | | |
| *C* | With *A* | 181·0 | 46·1 | | |
| | With *B* | 186·6 | 29·1 | | |
| | With *D* | 187·5 | 48·5 | 1·5 | N.S. |
| | With *A*, *B* | 206·2 | 96·4 | | |
| | With *A*, *B*, *D* | 214·5 | 39·6 | | |
| *D* | With *A* | 244·8 | 52·1 | | |
| | With *B* | 273·0 | 44·3 | 3·18 | 0·05 |
| | With *C* | 272·7 | 87·3 | | |
| | With *A*, *B*, *C* | 274·1 | 41·3 | | |

*Conclusion*

The only general conclusions suggested by our data so far are that within any one conversation individuals tend to maintain a certain level of speech rate, which may be an indication of a general level of stimulation either aroused on the occasion of this conversation, or existing at the time and maintained during its course. This level of speech rate is specific to the occasion, varying for any one individual from single occasion to single occasion, whereby the person of the interlocutor or the combination of a group are only one factor in the variation. (That it might at times be an important factor is suggested by the fact that all speakers accelerated when taking part in a general group discussion with *D* present.)

Other factors have so far not been investigated, but one would imagine that the level of speech rate on any one occasion is a reflexion of a number of stimuli, apart from the interlocutor, and that the effect of the latter would have to be broken down into effects of his social status in relation to the speaker, of his personality in general, and of his speech behaviour in terms of what he says as well as of formal characteristics of speech. The stimuli, apart from the interlocutor might include biological wellbeing, mood, interest aroused by the subject of conversation, as well as the degree of his familiarity with it.

However, over and above its fluctuations from occasion to occasion, speech rate differentiates between individuals. All the subjects investigated have shown themselves relatively limited in the degree to which they raised or lowered their speed of talking for

any length of time. The fast speakers remained consistently fast and the slow speakers consistently slow.[1] This is in keeping with the author's previous results based on doctors and patients in psychiatric interviews.

(iii) *The relation of speed and length of utterance*

The third question asked was what bearing the length of an utterance (or the number of syllables spoken) has on the speed of talking. Having excluded the very short utterances, i.e. those consisting of less than five syllables (it is obvious that rates of speech output for these could not hope to be accurate), the speeds of the separate utterances during each conversation were plotted on the abscissa against their respective lengths. The scale for the latter on the ordinate is in terms of the number of syllables contained in each utterance.

There were twenty-three scatter diagrams in all, containing an average of sixty-eight measurements (ranging from 49 to 93) per function. The type of relation which emerged is illustrated in Figs. 2–5, each being a combined scatter diagram for every individual using the readings from their several conversations.

The characteristic feature of these functions, namely their shape, is common to all twenty-three diagrams underlying them. The variations lie (*a*) in the location of central values of speeds on the abscissa, (*b*) in the width of scatter, and (*c*) in the height of the column, i.e. in the length of utterances to which each individual rises. The common elements are the following:

(i) Speeds for short utterances up to 20 and 40 syllables contribute most to the base of the curve. The latter spreads over the range from about 60 to 600 syllables a minute for the shortest utterances, i.e. from very slow to very fast, and narrows down as they grow longer.

(ii) Stability is gained at a length of utterance of about 100 syllables. At a length of utterance of 100 syllables the area of the curve has finally narrowed with all the utterances of greater length falling within a column.

(iii) Speed in short speech periods, while ranging from the extreme left to the extreme right of the distribution, is responsible for its skewness to the right. The right tail, as well as containing more measurements, is far longer.

(iv) High speeds of talking rarely occur in the case of utterances more than 60 syllables in length, and *never* in the case of those exceeding 100 syllables. The curve as a whole recedes towards the central value when the length of utterance is between 40 and 60 syllables.

Table 3 gives the means and standard deviations for different lengths of utterance (i.e. utterances containing 5–19, 20–39, 40–59, 60–79, 80–99, 100–199, and more than 200 syllables (this last includes utterances of up to 813 syllables in length). Figs. 5 and 6 show the diminution in the rapidity of speech and in the range of speeds for utterances of increasing length. In spite of the wide scatter along the scale of speed in the short utterances, speech rates are not evenly spread there, a central tendency being clearly discernible. An analysis of variance computed for the purpose of finding out at which length of utterance speech rate could discriminate between individuals (taking measurements from their entire conversation) showed that even in the very short utterances there

[1] Reading speeds based on five English tests of various types (prose and poetry) taken from *A*, *B*, *C*, *D* show this rank order of speech rate to be the same as for talking in conversations.

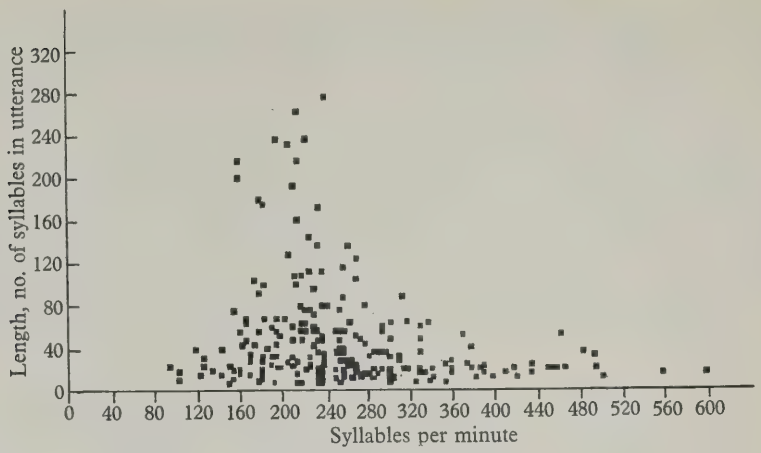



Fig. 2. Scatter diagram showing relation of speed and length of utterances for subject *A*.

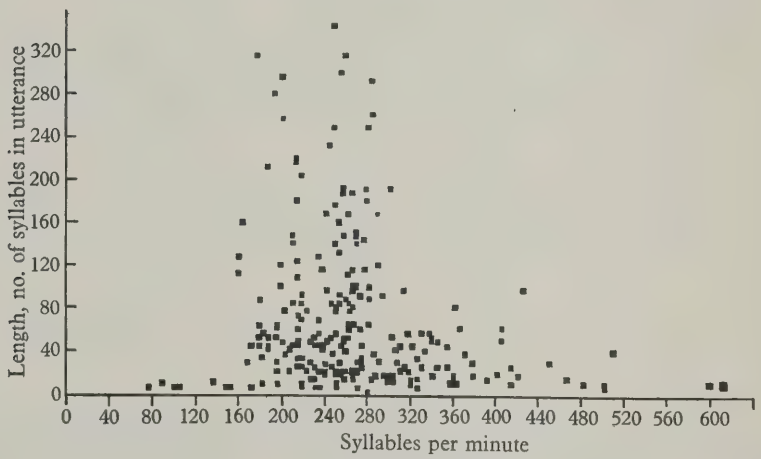



Fig. 3. Scatter diagram showing relation of speed and length of utterances for subject *B*.

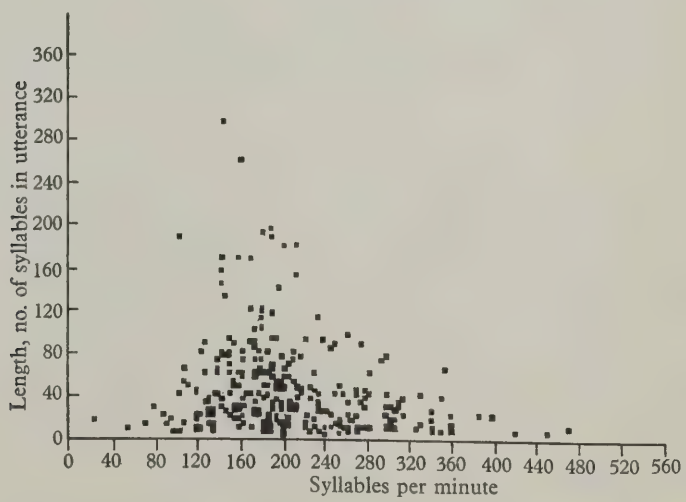



Fig. 4. Scatter diagram showing relation of speed and length of utterances for subject *C*.

was enough concentration around the central tendency for mean rate of speech to discriminate between the individuals *A*, *B*, *C*, *D* at a highly significant level of probability ($P$ 0·001).

Testing the discriminative capacity of speech rate for utterances of increasing length for the four records of the patients' interviews (this measurement came from only one

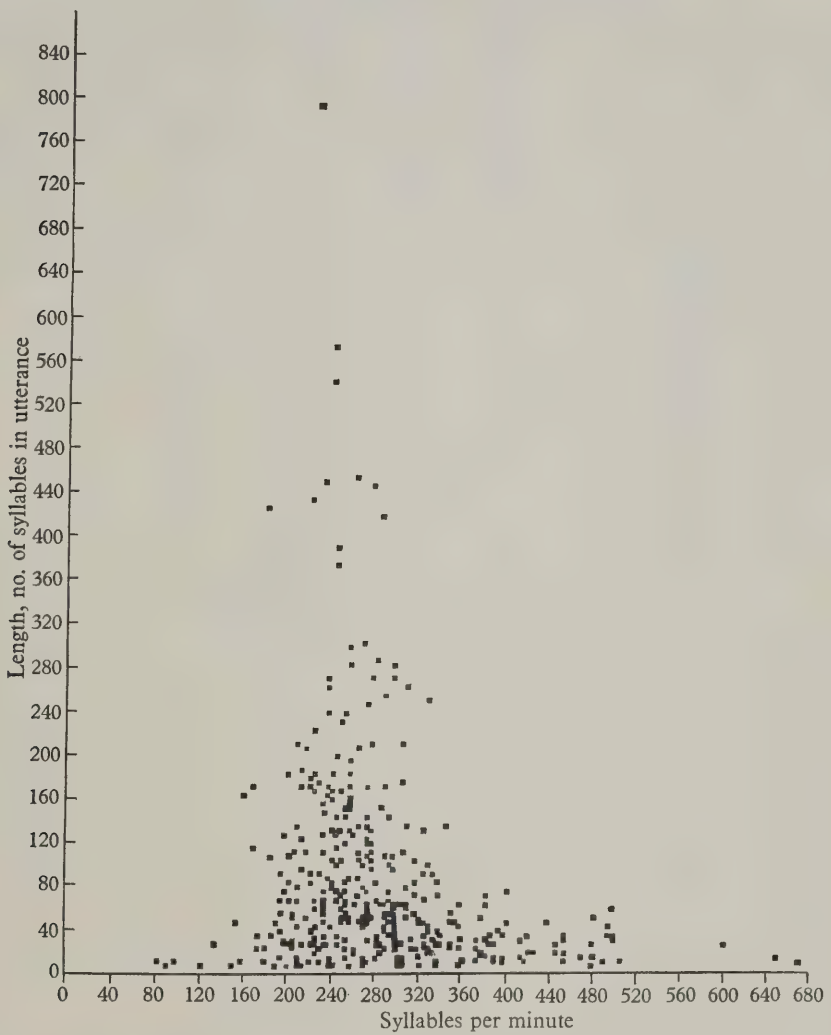



Fig. 5. Scatter diagram showing relation of speed and length of utterances for subject *D*.

interview), the mean speech rates for short utterances not exceeding 20 syllables proved incapable of discriminating between the four patients. But the speeds of talking for utterances of between 20 and 40 syllables in length, and also for those still longer, discriminated significantly as was mentioned above (Table 1).

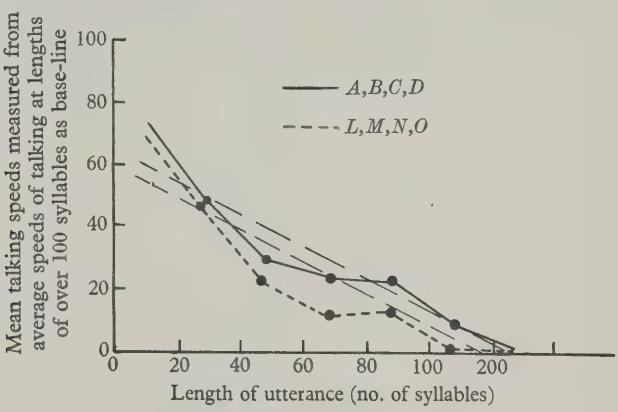
(iv) *Length of utterance and variability in speed of talking*

Fig. 6 shows the pooled standard deviations for utterances of different length of the members of the hospital staff *A*, *B*, *C*, *D*, and separately for those of the four patients

*L, M, N, O.* The regression line drawn through the points of the two curves shows a virtually identical slope, the standard deviation decreasing as the number of syllables uttered increases up to 200, at a rate of about 7:12. From there on the standard deviation

Table 3. *Means and standard deviations of speech rates (number of syllables per minute) for different lengths of utterances*

| | Syllables in utterance... | 5–19 | 20–39 | 40–59 | 60–79 | 80–99 | 100–199 | Over 200 |
|---|---|---|---|---|---|---|---|---|
| *A, B, C, D* | General mean | 272·3 | 254·2 | 239·2 | 233·2 | 237·6 | 230·8 | 229·8 |
| | S.D. | 108 | 79·7 | 71·2 | 56·4 | 53·9 | 40·8 | 31·0 |
| | No. of utterances | 319 | 315 | 218 | 119 | 84 | 165 | 78 |
| *L, M, N, O* | Mean | 264·6 | 244·4 | 219·1 | 207·9 | | 206·5 | |
| | S.D. | 99·7 | 53·9 | 45·6 | 31·3 | | 30·0 | |
| | No. of utterances | 109 | 85 | 46 | 50 | | 73 | |



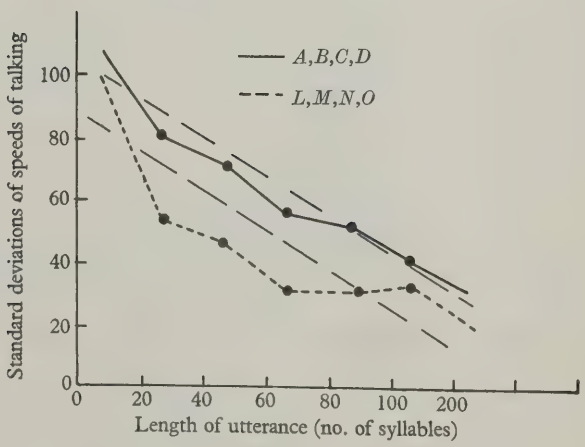




Fig. 6.

remains relatively constant. This is so in the case of the normals, *A*, *B*, *C*, *D*, as well as in that of the patients, with the difference that for the latter the scatter diminishes more rapidly, and constancy is attained with shorter utterances than in the case of the normal group. Whether this is due to the fact that a greater number of measurements exists for

the normals, or whether it may be a characteristic of neurotics that they are unable to maintain high speeds as long as normals is for the present a matter of speculation. However, the unequivocal nature of the general tendency of this relation should enable whoever wishes to investigate the question to answer it with a high degree of precision.

### (v) *Length of utterance and mean speed*

As mentioned above, a similar decrease with increasing length of utterance was observed in the means of speech rates calculated separately for utterances of the following lengths: ontaining 5–19, 20–39, 40–59, 60–79, 80–99, 100–199, and more than 200 syllables. Again, this was so for all records of normals and patients alike, irrespective of the individual differences in mean speech rates which had proved highly significant. When mean

Table 4. *Means and standard deviations of rates of talking for different lengths of utterances based on all speech samples recorded of A, B, C, D, and on interview recorded of L, M, N, O*

| | *A, B, C, D* | | | *L, M, N, O* | | |
|---|---|---|---|---|---|---|
| Length of utterance | No. of utterances | Mean | S.D. | No. of utterances | Mean | S.D. |
| 5–19 | 319 | 272·3 | 108·0 | 109 | 264·6 | 99·7 |
| 20–39 | 315 | 254·2 | 79·7 | 85 | 244·4 | 53·9 |
| 40–59 | 218 | 239·2 | 71·2 | 46 | 219·1 | 45·6 |
| 60–79 | 119 | 233·2 | 56·4 | 50 | 207·9 | 31·3 |
| 80–99 | 84 | 237·6 | 53·9 | | | |
| 100–199* | 165 | 230·8 | 40·8 | 73 | 206·5 | 30·0 |
| 200 and over | 78 | 229·8 | 31·0 | | | |

* Owing to the decrease in the frequency of utterances with increase in length, the speech rates underlying the means of utterances longer than 100 syllables were grouped into larger intervals.

speech rates were plotted for utterances of different length the slope of the regression line again showed little variation for the different individuals. To demonstrate this, measurements of mean speech rates were again pooled into two groups, one for normals and the other for patients. The different speed levels for the different individuals were equalized by taking the mean talking speed for the longest utterances (over 200 syllables) for each individual as his base-line or zero-point, which was subtracted from mean talking speeds of all the shorter utterances (six more classes of utterances). Fig. 5 shows that not only the angle of the slope but the shape of the curve of means as they decrease towards the basic mean speech rate (i.e. the talking speed with the greatest stability or smallest standard deviation) is remarkably similar in its characteristic way for normals and patients. It should, of course, be kept in mind that the high mean speeds of the short utterances cover a very wide range and that only for the longer utterances are the mean speech rates representative of the real values. As to the shape of the curve it seems as if the utterances of different length fell into three or four classes: (1) the very short utterances of up to 20 syllables and the next short ones of up to 40 syllables length with mean speech rates of 272·3 syllables a minute, and 254·2 syllables a minute, with a scatter of S.D. = 108·0 and 79·7; (2) utterances of middle length, that is those containing between 40 and 100 syllables, whose mean talking speeds seem to form a plateau in the curve and whose scatter has considerably decreased in the same way; and (3) long utterances of over 100 syllables' length, with a somewhat slower rate of talking having gained final stability. These groupings suggest that the different types of utterances may represent different functions in interaction. For the most part, retorts, expostulations and interjections would seem

to make up the two groups of short utterances, conversational exchanges the middle group of utterances, and the more considered statements, reports, deliberations, outpourings or diatribes of greater length would constitute the last group. It should be mentioned in this connexion that individuals differed considerably in the proportions of their total number of utterances assignable to the groups of utterances listed above It so happened that in our sample the normal as well as the neurotic subjects divided, in respect of length of utterance, into two fairly distinct types of talkers, as Table 5 shows. *A* and *C*, whose utterances are mainly short, are also the slow speakers in the group; whereas *B* and *D*, who usually speak at length, are also those with considerably higher mean speeds of talking (*D* 265·6 and *B* 240·0, *A* 226·5 and *C* 191·3). It is also of interest that the differences lie not in the utterances of medium length but in the short and long ones, *A* and *C* concentrating on the former, i.e. mainly responding and interjecting, while *B* and *D* show a much greater degree of staying power in talking.

The corresponding distribution of utterances of different length for the four patients showed a similar dichotomy, as shown in Table 5.

Table 5. *Proportions of utterances of different lengths in total output of A, B, C, D and L, M, N, O (in percentages)*

| Length of utterance | *A* | *B* | *C* | *D* |
|---|---|---|---|---|
| 5–19 | 33 } 57 | 20 } 40 | 29 } 58 | 18 } 40 |
| 20–39 | 24 } | 20 } | 29 } | 22 } |
| 40–99 | 33 } 43 | 32 } 60 | 33 } 42 | 25 } 60 |
| 100 and over | 10 } | 28 } | 9 } | 35 } |
| | *L* | *M* | *N* | *O* |
| 5–19 | 20 } 46 | 23 } 41 | 37 } 56 | 38 } 63 |
| 20–39 | 26 } | 18 } | 19 } | 25 } |
| 40–99 | 29 } 54 | 26 } 59 | 24 } 43 | 23 } 37 |
| 100 and over | 25 } | 33 } | 19 } | 14 } |

## VI. Discussion

To gain a better understanding of the implications of the phenomena described above it may be useful to contemplate the uniform way in which the rate of talking fell off markedly as utterances became longer. The fact that on no occasion were longer utterances spoken at high speeds suggests that a physical as well as a psychological factor is here operating as an impediment; in other words, the high rates of word output sometimes achieved in short bursts seem, under ordinary circumstances, physically impossible to maintain for any length of time. As we have seen, short utterances were not necessarily always faster than long ones, but in the case of the former the range of speed was wider from very slow (60 per min.) to very fast (600 per min.). What discriminates the short from the long utterances then is not only the rates at which they are spoken, but the inherent difference in the amount of freedom to vary speed. The latter is partly dependent on the physiological mechanism involved in speech production. Respiration in particular, which is the source of energy maintaining the output of speech, must be taken into account (Stetson, 1928). The production of speech requires a renewed column of exhaled air for extended vocalization and articulation. While the rate and rhythm of respiratory movements can be modified at will, the exercise of voluntary control over breathing is strictly limited by the respiratory reflexes which serve the more vital and primary functions of the organism (Travis, 1931). As it is biological necessity to interrupt the stream of speech

at more or less regular intervals in order to get air into the lungs, repeated inspirations which, however abbreviated, take time, will affect the durations of long utterances.

Utterances of short duration, on the other hand, may to a considerable degree escape the retarding effect of repeated respiratory action, and gain time by suspending breathing for short periods.

Apart from this physiological limitation to the pace at which speech is produced, there are mental processes involved in the production of speech; namely the need for conceptualizing, integrating, and phrasing, in other words the process of thinking involved in utterances of greater length which is generally more complicated. While the economy of breath control on the physical side, and the integrative processes of language formation on the mental, are factors setting limits to the rate of word output, factors having the opposite effect are also suggested. Examination of the records of the talking speeds of partners in conversation showed that utterances spoken at high speed by one speaker either followed or were followed by fast utterances, or started a series of quick exchanges. Another occasion during which the rate of talking was speeded up greatly was when an interlocutor interjected a remark into a breathing or thinking pause in the stream of an utterance before the speaker had come to an end. In such a case, which was a frequent occurrence in the discussions of *A*, *B*, *C*, *D*, the usual consequence was that after the interruption the rate of talking rose, often quite steeply, only to slow down again as the interlocutor relapsed into passivity. The speaker who interrupted, unless his interjection was very short, also, as might be expected, spoke faster than usual, having to fit his utterance into the pause of the other's stream of words.

The difference in the conditions of interaction under which short interjections or lengthy statements are made may be described as one between participation and sole possession of the field. And the object of participation or possession in speech situations is time, the dimension in which linguistic behaviour progresses. In a conversational situation, time is a quantity to be shared between the partners, and the more spontaneous such a situation is, the more open is left the manner (i.e. the more subject to the interaction of the interlocutors) in which this time for talking will be distributed between them. The problem of the distribution of time in interactional situations becomes critical when the demand for it exceeds the supply, i.e. when the partners in a conversation have the urge to speak, either simultaneously or when the speaker's urge for talking is so disproportionate as to prompt him to encroach beyond the quantity of time which his partner is prepared to forego. The fact that in our sample those who said most also spoke the fastest, seems from this angle to become easily understandable. For loquacious persons, time is in short supply, particularly when their conversational partners also have a lot to say. Only by increasing their word output per time unit can they manage to release material pressing for verbal expression. The acceleration in an individual's rate of talking may thus be taken as a measure of the intensity of this pressure towards verbal expression. Where both partners in a conversation seek this release simultaneously, the result is a clash of conversational actions, i.e. simultaneous talk, rapid bursts of speech, a series of interruptions and quick short exchanges. The clash manifesting itself in these forms is over the possession of time, or more precisely of a particular stretch of time. The circumstances in which acceleration in the rate of talking was observed, suggests that it is a particular form of action by which either the speaker's time is defended against his partner's encroachments, or the co-speaker's time appropriated or invaded while he still wishes to

continue talking. That an increase in the number of interjections and a cut in the time available can have the effect of speeding up the rate of talking to a spectacular degree has already been demonstrated in other research (Goldman-Eisler 1954). At which point time appears to a speaker to be in short supply is also a matter for his own subjective assessment, and the speed of talking would be an indication of subjective time as experienced by the speaker. This, one might assume, will be a function of the quantity of material that is urging to be expressed, the degree of urgency felt, the objective amount of time available, and, more basically, of time perception as determined by physiological and psychological processes (Lecompte de Nouy, 1936; Schilder, 1936).

In the light of the above considerations it would appear, then, that short utterances in conversation being relatively unhampered by the functional necessities of the physical and mental processes involved in speech production, and therefore being free to cover a very wide range of speeds, will have a much greater capacity than long utterances to reflect the intensity of interaction.

Long utterances, on the other hand, represent a different aspect of the conversational situation. They are the result of a speaker having taken the floor, and are dependent on sufficient time being available. They are conditional, too, on the passivity, whether voluntary or enforced, of the interlocutor, enabling a speaker to continue talking at some length. The speaker thus talks, so to speak, in his own time.

It is the speed of talking in the long utterances, therefore, which indicates to what extent a speaker considers time to be his 'own'. In other words, the speed of talking in long utterances which has emerged as having great stability within any particular conversational situation, and as being characteristic for individuals, although flexible with varying situations, may turn out to be a measure of an individual's subjective time in any particular situation, i.e. of how much time he feels he has, and how secure he feels in its possession.

Further investigation is, of course, necessary to corroborate the suggestion of this link between rate of talking and time sense.

## VII. Summary

1. The speed of talking (or speech rate) had previously been shown to be a consistent as well as a highly sensitive aspect of conversational behaviour.

2. This paper deals (*a*) with the question of the speed of talking measured as the number of syllables a minute for the separate utterances as distributed within a single conversation, (*b*) with individual differences and the situational determination of the rate of speech, and (*c*) with the relation of speed of utterance to length of utterance.

3. The material was based on transcribed speech recordings of ten conversations and group discussions involving four normal adults (three men and one woman) of academic status and four interviews of young female neurotics yielding altogether twenty-three records. The conclusions are based on a total number of 1299 utterances.

4. The frequency distributions of speech rates for the different individuals in different situations had the common characteristic of being skewed positively, and six of the eight distribution curves were leptokurtic.

5. Speech rate discriminated between individuals at a high level of significance. It fluctuated for any one individual at varying degrees in the different situations, but within the intra-individual range. No systematic effect on speech rate was exercised by

persons, the interlocutor emerging rather as *one* of the factors affecting the speed of talking.

6. The relation between speed and length of utterance was illustrated by scatter diagrams for each subject, based on pooling the utterances from all their conversations. They show that the speeds of utterances, which lie along the base of the distribution, are constituted entirely of short utterances, rarely containing more than 40 syllables, while the body of the distribution includes all the long utterances. Length thus impinges upon speed of utterance in the following ways.

(*a*) The range of speech rates is relatively great for short utterances, but narrows as the utterances grow longer.

(*b*) Stability is gained at a length of about 100 syllables. Speech rates for utterances longer than that are confined within the area of a column, its base being to the left of the mid-point, and its height representing lengths of utterances.

(*c*) Fast speech rates occur rarely at lengths above 60 syllables per utterance and *never* above 100 syllables.

(*d*) Means and standard deviations of speech rates for utterances of different lengths decreased with the increasing length of utterance: the stabilization of speed in talking being in the direction of a slowing down.

7. In spite of the wide scatter, speech rates for short utterances as well as long ones discriminated between individuals with high significance.

8. The curve of decreasing mean speeds for increasing mean lengths is in shape and slope very similar for the four normal and the four neurotic subjects. It points to three and possibly four types of utterances according to their lengths, suggesting as many different functions in interaction.

9. Further analysis as to the meaning of the rate of talking from its relation to length of utterance, pointed to a link between speech rate and sense of time.

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# AN EXPERIMENTAL STUDY OF COMPARATIVE JUDGEMENTS OF TIME

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## I. Introduction

The seeming duration of outer or private events is perhaps the most fundamental dimension of human experience. In spite of this the literature on the subject is disappointing. Woodrow (1951) remarks in his recent review that data accumulated by various investigators have two outstanding characteristics, one being their mutual inconsistency and the other their 'mentalistic' nature. It is not clear whether he considers the second characteristic a merit or defect. The studies considered by Woodrow have been very largely confined to the comparison and reproduction of intervals up to about 10 sec. in duration, but there have also been introspective accounts of single intervals of three or four minutes in duration. Woodrow's review scarcely does justice to the subject, for he overlooks the basic problems of the psychology of time and the frequently reported temporal phenomena which are of universal interest, for example, the phenomenon that the years seem to pass more rapidly as we grow older (James, 1901). Nor does he refer to the only serious book (Sturt, 1925) entirely devoted to the psychology of time in which many of these questions are raised and discussed in the light of evidence of a kind which he does not consider.

A different approach to the study of time was introduced by Piéron (1952) and his students in France in 1929. Piéron's view is that units of subjective time are functions of the velocity of deep-seated organic processes which determine metabolism and other vital activities. As is well known, this approach has been developed by Hoagland (1935) in the United States, but important as the work undoubtedly is, it has only a limited bearing on the experience of time, principally in elucidating what might be called the organism's 'chronostatic' mechanisms. In the present study we attempt to explore the subjective duration of long intervals of time.

## II. Aim of the experiment

Our aim in this experiment has been to discover the manner in which the seeming duration of time since different past events varies with the lapse of chronological time since these events. There is at least one hypothesis which might lead one to expect a non-linear relationship between chronological intervals, as they extend further and further into the past, and our judgements of them, such judgements presumably showing a relative contraction. On the basis of Weber's law we might expect quantitative judgements of our experience of time intervals, at any particular age, to increase linearly as the chronological intervals increase logarithmically.

## III. SUBJECTS AND PROCEDURE

There were seventy-two children, thirty undergraduates, and 130 students of Adult Classes. These were divided into groups as shown in Table 1. All groups included both sexes. Twenty-two of the twenty-four children of mean age 13 years 6 months were studied under two sets of conditions and therefore appear twice in Table 1.

The essence of the procedure was that subjects marked off lengths of a line to represent duration of past intervals. Subjects experienced no difficulty in this procedure. This is not surprising if we consider that in everyday language words denoting length are used to refer to periods of time (e.g. a short time, a long time) and also that, in respect of short intervals of time, judgements of spatial and temporal intervals are inseparable (Cohen, Hansel & Sylvester, 1953).

The subject was given a line 10 in. long on a sheet of paper. He was asked to imagine that this line represented his life from 'birth' to 'now', and to mark off a length representing a given interval. A different line was given for judging each interval. Seven different intervals were presented in random order, this procedure being repeated four times. In the second and fourth presentations, the end-points of the line representing 'birth' and 'now' respectively were reversed. The subjects were carefully instructed to indicate, by marking off a length of the line, how long a given interval *seemed* in relation to the total past life or, as in part of the experiment, in relation to another fixed interval (e.g. 1 year).

In order to make our procedure clear, we must make a distinction between (i) an interval since an event ('How long does it seem since event *X* occurred?'), and (ii) the point on a time scale when an event occurred ('When did the event *X* occur?'). We are concerned with (i) only. In this case the subject is asked to indicate or represent his 'feeling' of the duration of an interval without conscious calculation of chronological time. It must be clear to him *when* the delimiting event occurred. In the second case, with which we are not here concerned, the subject would be asked to locate an event on a line representing his life, or part of it, to date, without necessarily knowing precisely *when* the event occurred.

The choice of birth as an anchoring point was made for two reasons, first it is definite and easily ascertainable, and secondly we assume that the whole of a person's past life is in an important sense a unity.

In the case of groups (v) and (vii) (consisting of students of Adult Classes) in Table 1, all the subjects were assembled together, and twelve intervals were presented in random order in a single series.

Because the experiments with the various groups had to be carried out on different occasions, and also because some intervals suitable for children (e.g. the interval since the end of the school holidays) were unsuitable for adults, the intervals chosen also had to vary.

The details relating to the groups and procedures are set out in Table 1.

## IV. RESULTS

The means and standard errors are set out in Table 2, and the means are plotted against the logarithms of the intervals in Fig. 1. The estimates are lengths marked off a line representing the entire life. We have calculated the medians, in addition to the means

because some of the distributions depart from normality and in such cases it may be argued that the means are not the most representative values of the variable. These medians are shown in Fig. 2. We return to this later.

## V. Interpretation of results

The figures in Table 2 indicate comparative judgements of time intervals at each age. The distributions of the estimates for any interval given in the table vary markedly as between the children and the adults. The estimates of the children are approximately normally distributed, whereas those of the adults are skewed and in some cases bimodal. We may infer from this that the skewness and bimodality are not attributable to the scale, and that under favourable conditions the estimates yield a normal distribution. The skewness and bimodality of the distributions for the adults may be due to the fact that in the older groups, a proportion of subjects make their estimates on a linear basis, as a result perhaps of their experience of the use of linear scales. As stated above, it is because of this skewness that the medians have been calculated. It should be noted,

Table 1

| | | Age | | Method | | |
|---|---|---|---|---|---|---|
| Group | Number | Yr. | Mnth. | No. of intervals | Times presented | Scale |
| (i) | 25 | 8 | 3 | 7 | 2 | 10 in. = life |
| (ii) | 23 | 11 | 10 | 7 | 2 | 10 in. = life |
| (iii)* | 22 | 13 | 6 | 7 | 4 | 10 in. = life |
| (iv) | 30 | 19 | | 7 | 4 | 10 in. = life |
| (v)† | 130 | 20–74 | | 11 | 1 | 10 in. = life |
| (vi) | 24 | 13 | 6 | 7 | 4 | 10 in. = 1 year |
| (vii)‡ | 7 | 46 | | 8 | 1 | 12 in. = 10 years |

* This group is also included in group (vi).
† In this group one interval was repeated as a check.
‡ The estimates are plotted in Fig. 4.

however, that the medians may be unduly influenced by the proportion of subjects making linear estimates.

For intervals up to about 6 months the form taken by the subject's estimates is such that as the actual intervals increase logarithmically his estimates increase in approximately linear fashion. Intervals beyond 1 year yield estimates which are approximately linear and go back to the point representing birth. In other words, the linear portion of the line is divided up in a way which would be arrived at by calculation. This linear effect is, indeed, obtained when subjects instead of being asked to judge apparent intervals are asked to locate historical events along the given line. It would seem, therefore, that by this method the subject is unable to appreciate comparative intervals greater than about 1 year from now and is only able to locate events.

Thus, the immediately preceding 24 hr., for example, seems a relatively long time in proportion to the preceding month, whereas there is no perceptible disproportion when comparing the previous 5 years with the previous 10 years. These latter judgements (i.e. for intervals beyond 1 year) are made in an entirely different fashion and seem unable to be formed without reference to dates or events in one's personal past to which dates can be assigned or age estimated.

Estimates of intervals between 6 months and 1 year are highly variable. It would seem that the estimates are sometimes arrived at by the method of shorter intervals and some

times by the method of longer intervals. This variability is clearly not due to scale errors because it appears in the estimates of groups of different ages for whom the interval of 6 months would be located on a different part of the scale. For the same reason the

Table 2. *Mean estimates (and standard errors) of intervals (in inches)*

(Scale 10 in. = 'life'.)

| Group of subjects | | (i) | (ii) | (iii) | (iv) | (v) | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Mean age (years) | | 8·42 | 10·83 | 13·5 | 19+ | 20–29 | 30–39 | 40–49 | 50–59 | 60–74 |
| Number | | 25 | 23 | 22 | 30 | 19 | 31 | 33 | 40 | 7 |
| Interval | | | | | | | | | | |
| Actual | As fraction of year | | | | | | | | | |
| 1 day | 0·003 | — | — | 0·9 (0·13) | — | 0·46 (0·17) | 0·35 (0·10) | 0·46 (0·09) | 0·56 (0·08) | 0·73 (0·38) |
| 1 wk. | 0·019 | 2·0 (0·27) | 1·9 (0·30) | 1·8 (0·32) | 0·7 (0·18) | — | — | — | — | — |
| 3 wk. | 0·058 | 1·9 (0·34) | — | — | — | 1·1 (0·26) | 1·0 (0·19) | 0·71 (0·09) | 1·3 (0·07) | 1·2 (0·29) |
| 8 wk. | 0·154 | — | 3·1 (0·57) | 3·1 (0·61) | — | — | — | — | — | — |
| 10 wk. | 0·19 | 3·6 (0·22) | — | — | 1·3 (0·19) | — | — | — | — | — |
| 4 mnth. | 0·33 | 5·9 (0·50) | — | — | 2·0 (0·19) | 1·2 (0·23) | 1·3 (0·24) | 1·1 (0·13) | 1·1 (0·17) | 1·2 (0·24) |
| 6 mnth. | 0·50 | — | 4·1 (0·54) | — | — | 2·4 (0·33) | 2·1 (0·29) | 2·2 (0·52) | 1·8 (0·45) | 2·0 (0·33) |
| 7 mnth. | 0·58 | 5·9 (0·52) | 4·9 (0·53) | 4·0 (0·50) | — | — | — | — | — | — |
| 9 mnth. | 0·75 | — | — | — | — | 2·1 (0·34) | 2·0 (0·58) | 2·4 (0·56) | 2·2 (0·55) | 3·7 (0·72) |
| 12 mnth. | 1·0 | — | 4·5 (0·38) | 4·5 (0·52) | 2·6 (0·22) | — | — | — | — | — |
| 1 yr. 3 wk. | 1·058 | 7·3 (0·41) | 4·3 (0·34) | — | — | 3·0 (0·32) | 2·7 (0·76) | 3·3 (0·57) | 3·3 (0·36) | 4·0 (0·57) |
| 3 yr. | 3·0 | 5·5 (0·40) | — | — | — | 4·6 (0·49) | 2·95 (0·50) | 4·2 (0·65) | 3·6 (0·27) | 3·5 (0·46) |
| 4 yr. 5 mnth. | 4·45 | — | 7·5 (0·27) | 5·9 (0·45) | — | — | — | — | — | — |
| 7 yr. 6 mnth. | 7·5 | — | — | — | 4·4 (0·44) | — | — | — | — | — |
| 8 yr. | 8·0 | — | — | — | — | 4·8 (0·43) | 4·1 (0·44) | 3·8 (0·60) | 2·6 (0·44) | 5·4 (0·63) |
| 8 yr. 6 mnth. | 8·5 | — | — | 8·5 (0·39) | — | — | — | — | — | — |
| 13 yr. 6 mnth. | 13·5 | — | — | — | 7·2 (0·26) | 6·7 (0·39) | 5·1 (0·32) | 4·7 (0·50) | 4·5 (0·62) | 5·5 (0·69) |
| 14 yr. | 14 | — | — | — | 8·0 (0·23) | — | — | — | — | — |
| 27 yr. | 27 | — | — | — | — | — | 9·0 (0·18) | 8·0 (0·17) | 7·3 (0·27) | 6·9 (0·44) |
| 35 yr. | 35 | — | — | — | — | — | — | 9·0 (0·02) | 7·4 (0·18) | 6·9 (0·50) |

logarithmic relationship for intervals up to about 6 months cannot be attributed to scale effects.

A further check on the possibility of the logarithmic effect being due to the scale was made by obtaining estimates (by the 13·5-year age-group) on a line representing 1 year from now. These estimates were compared with the corresponding estimates made on the

scale representing life. They were found to yield a good fit to the corresponding part of the curve fitted to the estimates made on the life scale. The comparison is shown in Table 3.

Table 3. *Mean estimates of intervals (in inches)*

| Interval | | Group (iii) (Scale, 10 in. = 'life') | Group (vi) (Scale, 10 in. = 1 yr.) |
|---|---|---|---|
| Actual | As fraction of year | | |
| 1 day | 0·0027 | 1·548* | 1·24 |
| 3 days | 0·008 | — | 2·46 |
| 1 wk. | 0·019 | 3·09 | 2·89 |
| 4 wk. | 0·076 | — | 5·31 |
| 5 wk. | 0·096 | — | 4·65 |
| 8 wk. | 0·154 | 5·33 | — |
| 3 mnth. | 0·25 | — | 6·55 |
| 7 mnth. | 0·58 | 6·88 | 6·66 |

* Figures in this column are multiplied by a constant (1·72).

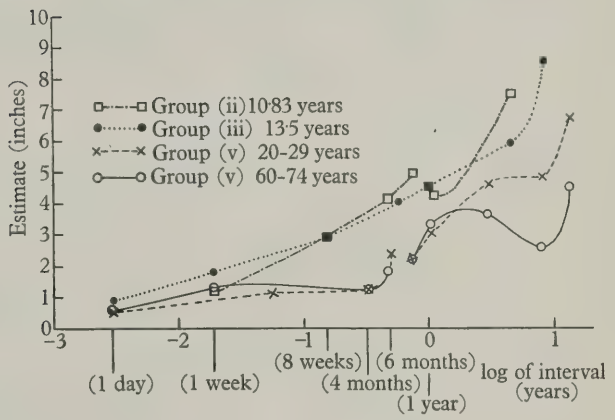


Fig. 1. Means of estimates.

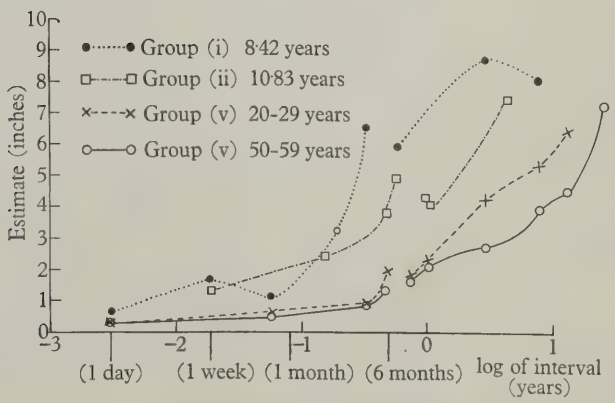


Fig. 2. Medians of estimates.

In Figs. 1 and 2 the curve representing the relationship between the intervals and the estimates shows a discontinuity at a constant point on the time scale ($Y$-axis), irrespective of where the point falls on the estimate scale ($X$-axis). This discontinuity thus appears

to be a function of the time interval and not of the estimate scale. The discontinuity cannot be attributed to the actual delimiting events chosen because these varied from group to group. Furthermore, the experiments extended over a period of 3 months so that the same event delimited different intervals for different subjects and the same interval was delimited by different events.

The suggestion that two types of judgement respectively are made for intervals up to about 4–6 months and beyond 1 year is supported by (i) the bimodality of the distribution of estimates in the intervening zone, and (ii) the increased scatter in this zone. Coefficients of variation are plotted to illustrate the variation in scatter and they decrease fairly smoothly with more remote intervals, except for the intervening zone where there is a pronounced departure in the form of a hump. These coefficients are shown in Fig. 3.

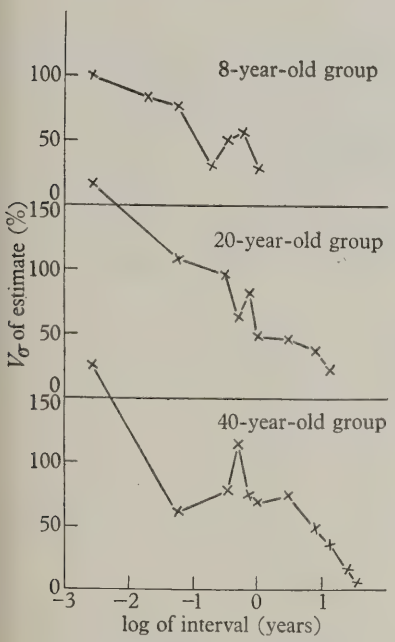


Fig. 3. Coefficients of variation at different intervals.

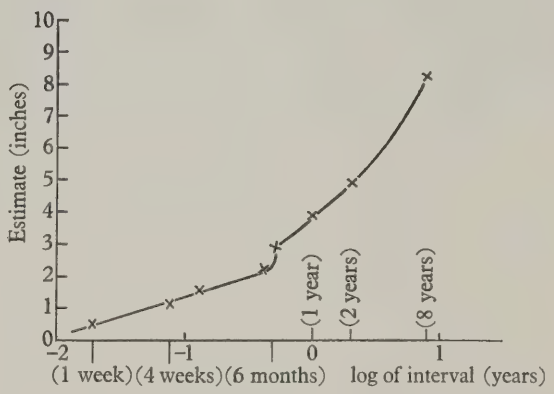


Fig. 4. Group (vii). Scale, 12 in. = 10 years. Mean estimates.

## VI. CONSIDERATION OF HYPOTHESIS

We now return to the initial hypothesis. If we plot the estimates against the logarithm of the intervals we obtain a reasonable fit to a straight line for intervals up to about 6 months from now. We may therefore conclude that the expectation, based on Weber's law, that a logarithmic increase in chronological intervals should yield a linear series of estimates of these intervals, is supported by our data only for past intervals up to about 6 months.

It is of course possible that the above effects may be accounted for by other hypotheses with which we are not here primarily concerned. One such hypothesis might be that the seeming length of an interval may be a function of the retention of activity during the interval and, as retention diminishes with time, there might be a corresponding contraction of judgement of time intervals.

## VII. Summary

We have attempted to extend the quantitative study of psychological time to intervals of months and years. The method employed is based on the assumption that (*a*) one's total past life is a unity which can be used as a standard of comparison, and (*b*) that estimates of past intervals can be represented spatially.

Our results suggest that, at any given age, estimates of intervals since given past events may be interpreted in accordance with the Weber principle up to a period of about 6 months. Beyond this period our method appears to yield a linear relationship only. This may be because people are unable to make direct judgements of intervals beyond 6 months, such intervals being calculated by them on a chronological basis.

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# SOME PROBLEMS OF ANOMALOUS COLOUR VISION

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## I. Introduction

This paper is an attempt to explain anomalous colour vision in a simple way and to meet as far as possible various difficulties and problems concerning it. These problems are complex and by no means settled at present, but many people are interested in them, and both from practical and theoretical points of view they are important. They will be treated in a general way here, the principal aims being to set forth, to co-ordinate and as far as possible to harmonize various contributions to the whole subject, but in the space available no attempt can be made to give its complete history or to deal with all the problems in full detail.

## II. The anomalous trichromat

Owing to the work of Helmholtz and many others, especially of Wright (1946), it may be taken as fact that those with normal colour vision can match in hue all colours in the spectrum, and, of course, also those outside it, with appropriate mixtures of three suitably chosen homogeneous lights, namely red, green and blue. If we allow the hue being matched to be desaturated with one of the matching hues, then satisfactory matches for saturation as well as for hue can be made. This has led to much discussion, because it calls for the use of 'negative' quantities of the matching colours in certain combinations. However, these negatives are not large, and might be satisfactorily accounted for simply as measurements of the degrees of dilution or desaturation which are produced by the over-lapping of the responses or functions of three primary types of process or receptor, none of which can be excited without one or both of the others being also excited at the same time, and therefore the negatives do not necessarily contradict a strictly trichromatic theory. In principle only three variables are being manipulated. At the same time, of course, there might be more primaries, and the negatives might be due to still more complex overlapping and/or to the presence of additional primary receptors, functions or processes.

The trichromatic theory, however, assumes the smallest practicable number of kinds of receptors or processes, namely three, but the facts of trichromatic colour mixing could equally well be satisfied by a theory that there were more, say four, with Hering and Houstoun (1932), or seven, with Edridge-Green (1891), and Hartridge (1948). The trichromatic theory states that, however many kinds of receptor or function may be at work, their action must be such that it can be represented as if there were only three, though it must be added that some recent work by Hurvich & Jameson (1951), Pickford (1951), and Piéron (1952), tends to confirm many long-standing suspicions that there must be independent receptors contributing to brightness.

When adequate experiments have been done on sufficiently large numbers of people it is not impossible that a very small proportion will be found who require more than three primaries to match all colours. A small proportion of people appear to be dichromats, namely, less than 1·5% of men, of whom about 1% are protanopes and about 0·5% are deuteranopes, and these proportions are far smaller among women. It is not easy to be sure that a given defective is absolutely dichromatic, because of difficulties in testing, and because the result of a test depends partly on the area of the retina stimulated and the intensity of the stimulus, but to the best of our knowledge the small proportions mentioned are dichromatic. Of the remainder of the population of Britain, about 92·5% of men and 99·5% of women are normal trichromats, but there is a small amount of variation among them which is not detected at all by ordinary 'colour blindness' tests (Pickford, 1949*a*,*b*, 1950, 1951). The remaining 6%, approximately, of men, are anomalous trichromats, and they fail on the Ishihara Test, like dichromats.

## III. The Rayleigh Equation

Rayleigh (1881) described an experiment in which subjects were asked to match a variable mixture of red and green lights against a standard yellow. Lithium red, thallium green and sodium yellow have become the standard colours to be used for this purpose. He described three ways in which the experiment could be done, and he found that among twenty-three male observers five, three of whom were his brothers-in-law, required more green while two required more red than the majority. It was to these exceptional subjects, who were defective without being dichromats, that the term 'anomalous trichromat' was first applied. One apparatus which he used was subsequently employed by Schuster and by Edridge-Green (1920), both of whom tested larger numbers of subjects. Since this time many workers have used the anomaloscope, as the apparatus has come to be called, and in Houstoun's (1932) form of it the variable proportions of the red-green mixture were controlled by nicol prisms, while in Nagel's, which has become an established pattern (Lohmann, 1913; Willis & Farnsworth, 1952), the mixture is controlled by varying the widths of two slits in inverse proportions. Collins (1929) and subsequently Pickford (1951) showed that similar results could be obtained with rotating paper disks. Since essential characteristics of anomalous trichromats always concern the relative sensitivity to red and green in matching a yellow light, the Rayleigh Equation has become the most satisfactory test for their defects. Several types of anomaloscope have been devised (Willis & Farnsworth, 1952; Crawford, 1951*a*, *b*; Pickford, 1951).

Much research has been devoted to the colour vision of anomalous trichromats, but it appears that their characteristics are still to some extent in doubt, and their relative frequencies in various populations tested have not been sufficiently studied. When Holmgren's Wools test was the usual test for colour blindness, the total number of defectives among men appeared to be about 5%. With the use of Stilling's Tables and the Ishihara and other pseudo-isochromatic plates, the number rose to about 7·5% or more. These plates fail all the anomalous trichromats as well as the dichromats, but the true proportion of anomalous subjects must be settled by the use of the Rayleigh Equation with some form of anomaloscope, because pseudo-isochromatic plates do not sufficiently discriminate between the types (Chapanis, 1949*b*; Hardy, Rand & Rittler, 1945*a*–*c*, 1949; Pickford, 1949*b*, 1950, 1951). The so-called 'exaggerated simultaneous contrast' of the anomalous is probably an effect of colour naming, of perceiving rather than of sensation

tself, or of judgement. It is not a reliable guide (Chapanis, 1949*a*; Pickford, 1951), and annot be employed to diagnose the anomalous as suggested by Lohmann (1913) in his echnique for using the Nagel card-test. The importance of adequate tests has already been emphasized (M.R.C. (1933); Riddell *et al.*, 1949; Pickford 1951; Wright, 1953) and vill not be discussed further here.

## IV. Types of red and green anomaly

Houstoun (1932) pointed out that there was a difficulty in the distribution of red and green deviations in the Rayleigh Equation. Some deviations fall within the normal curve and others fall outside it. He said that it would be best to reserve the term 'anomalous' for those subjects whose deviations fall right outside the normal curve. If this is done, the others may be called red and green deviants if they have deviations of about two to three times sigma (Pickford, 1949*a*, 1951). Houstoun also pointed out, on the basis of another set of experiments, that there is a proportion of subjects whose colour discrimination is good but different from that of the majority. He called these 'colour different', and possibly they are to be identified with the strictly anomalous subjects, whose matching ranges in the Rayleigh Equation are not at all or not much increased, but their deviations are of the order of six or seven times sigma, which is very far outside the normal curve.

It is increasingly clear that the concentration of attention upon deviation in the Rayleigh Equation, and the desire to give each subject a definite 'quotient' of red to green required to match the yellow standard, has to some extent obscured the facts. Walls & Mathews (1952) express clearly the views adopted by many workers, who are supporters of the trichromatic theory, namely, that all types of anomalous subjects must reject the normal Rayleigh Equation, and that with increasing defect, increasing deviation either to the red side for the protanomalous subjects, or to the green side for the deuteranomalous, leads through extreme anomaly, though not necessarily in a continuous way, to complete dichromasy. Nothing could be further from the truth.

An anomaloscope employing monochromatic colour filters, was used to measure systematically by an efficient psycho-physical technique the subject's average choice of red-green mixtures to match the yellow, and also the complete ranges of those matches, on a large number of subjects chosen at random and on many defectives, using several forms of test (Pickford, 1949*a*, 1951). It was apparent, as Edridge-Green (1920) and Franceschetti (1928) had observed before, that subjects differ not only in the deviations of their average red-green mixtures to one side or the other, but also in the magnitudes of their matching ranges. The relationship between deviation and matching range is complex and variable. Sloan (1950) and Crawford (1951*a*, *b*) have both described or mentioned the difference between the strictly anomalous who have normal or almost normal matching ranges but big deviations, and other incomplete trichromats who have greatly exaggerated red-green matching ranges either with or without deviations of six times sigma or more. The increases of matching range in question are very great, often as much as fifteen or twenty times the normal, and amount to 'colour blindness', though the subjects in question are not dichromats. Further work has been done along the same lines by Willis & Farnsworth (1952).

It is clear now that some major red-green defectives simply make a very abnormal Rayleigh Equation, and may be called truly anomalous or colour different. They fail unequivocally on the Ishihara Test. Others have greatly exaggerated red-green matching

ranges when the yellow standard is kept at the same brightness for them as the matching mixture, but not as great as those of the true dichromats, who can match yellow with red or green or with any mixture of these colours, provided again that the mixture is of the same brightness for them as the yellow standard. The partial trichromats with exaggerated matching ranges may have strong deviations to the red or green side as well, but some do not. They also fail on the Ishihara Test.

The problem of the nature of the relation between matching range and deviation is complex. All those who are complete dichromats, of course, have no measurable deviation, but those who are almost dichromatic may have mid-points or deviations nearer the normal in the Rayleigh Equation than those who are anomalous trichromats and have small ranges and big deviations. This is probably difficult to understand on the trichromatic theory, but on the bipolar theory of Houstoun it is perfectly clear. The anomalous in the narrow sense do not form a series continuously graduated in frequency and deviation between trichromats and dichromats, nor are the deviations of those partial trichromats who have exaggerated matching ranges graduated evenly in this way either. Some subjects who have a lesser degree of trichromasy have relatively smaller deviations, and vice versa. This is also easier to understand on Houstoun's than on the trichromatic theory.

In addition there are two further subgroupings which are clear. First, among the 'red different' or protanomalous in the narrow sense, some have the red end of the spectrum, shortened or darkened to a statistically significant extent, while for others it is of normal length or brightness. Secondly, in the group of partial dichromats or moderately dichromatic subjects of the deuteranope class, who have exaggerated red-green matching ranges in the Rayleigh Equation, some have characteristically marked green deviations and reject the normal match, while others do not. It is clear, therefore, that deviation in the Rayleigh Equation is large when we are dealing with the strictly anomalous subjects, but becomes less and less as matching range increases, though these changes sometimes occur in a stepwise manner, until the condition of dichromasy is reached, and then deviation is zero and matching range infinite. This is very different from what has been assumed by some workers on the basis of the trichromatic theory (Walls & Mathews, 1952). Their assumption is that deviation must increase continuously until dichromasy is reached.

## V. Terminological problems

The terminological difficulties are considerable. The best plan is to begin by confining the terms 'protanomalous' (PA), 'red anomalous' or 'red different', on the one hand, and 'deuteranomalous' (DA), 'green anomalous' or 'green different', on the other, to those partial trichromats who have good red-green discrimination, namely normal or almost normal matching ranges, but who have big deviations of the order of six or seven times sigma in the Rayleigh Equation. Then the terms 'extreme protanomalous' (EPA) or 'moderate protanope', and 'extreme deuteranomalous' (EDA) or 'moderate deuteranope', may be used for those partial trichromats who have exaggerated red-green matching ranges with or without large deviations. Those mentioned previously who are in this group but reject the normal equation may be called 'deviant deuteranopes', and are probably the same as the 'anomalous deuteranopes' of de Vries (1949). Similar subjects are occasionally met with among moderate protanopes, but are very infrequent and may be overlooked at present.

A considerable difficulty is created by the fact that the EDA group, as defined above, nd to a lesser extent the EPA group, is rather too variable for all its members to be alled 'extreme anomalous', after we exclude the deviant deuteranopes. It is true that ome EDA and EPA subjects are almost dichromatic, and, of course, have very small nean deviations. They sometimes have mid-points on the 'wrong' side of the normal natch. It is reasonable to call such subjects and the deviant deuteranopes 'extreme euteranomalous' or 'deuteranomalous', but there is a proportion who have much smaller natching ranges, though still perhaps ten times as great as those of the average normal ubject, and also rather small deviations or none at all. These subjects are clearly ot to be classed as DA (or as PA if they are found in the protanope series), but their efects are much less than those of the obviously extreme anomalous. In spite of the roubles which might be caused by multiplication of terms, it seems that there would be n advantage in speaking of 'lesser' and 'greater' defectives in the ordinary EDA group t least, and possibly in the EPA group as well, though they are statistically continuous.

The terms 'protanope' (P), and 'deuteranope' (D), may be reserved for true dichromats,

Table 1. *Major red-green defectives, totally sex-linked in heredity*

| | Incomplete trichromats | | Complete dichromats |
|---|---|---|---|
| Protans | 1. Protanomalous, red anomalous or red different (PA): (*a*) with red of normal brightness; (*b*) with darkened red | 2. Extreme protanomalous or moderate protanopes (EPA), not subdivided here | 3. Complete protanopes, or red blind (P) |
| Deutans | 4. Deuteranomalous, green anomalous or green different (DA) | 5. Extreme deuteranomalous or moderate deuteranopes (EDA): (*a*) deviant; (*b*) ordinary (lesser and greater) | 6. Complete deuteranopes, or green blind (D) |

xcluding 'tritanopic' dichromats, who are undoubtedly very rare (Wright, 1952). Nevertheless, the distinctions between the most extreme of the EPA and the P subjects n the one hand, and between the most extreme of the EDA and the D on the other, will ot always be easy to make.

A criterion for the distinctions between PA and EPA, and between DA and EDA, hich might be thought important, is that the PA and DA might confidently reject the ormal match in the Rayleigh Equation, always provided that brightnesses are equated or the subject in question at that point. As already mentioned, however, all deviant euteranopes and a few EPA subjects also reject it, and so this criterion cannot be used. The distinctions between PA and EPA and between DA and EDA therefore turn mainly n the size of the matching range, which is very small in PA and DA subjects but large or ery large in EPA and EDA. The PA and DA subjects form homogeneous groups with arge deviations and small matching ranges, whereas the EPA and EDA subjects always ave large or very large matching ranges, and they are somewhat heterogeneous groups.

The terms 'protan', for all protanopic subjects, and 'deutan' for all deuteranopic ubjects, introduced by Farnsworth, will be convenient, and no difficulties will be raised y employing them in these ways, as they have been used by Sloan (1950) and Chapanis 1951), and by Willis & Farnsworth (1952).

Table 1 shows the classification of major defectives discussed in this article, and may

help to clarify the terminological difficulties. Minor red-green and yellow-blue defectives, who may be called deviant and colour weak trichromats, and who pass the Ishihara Test with a small number of errors or no errors at all, and major blue-yellow defectives, have been omitted.

## VI. Statistical problems

The question arises in exactly what ways the so-called anomalous trichromats of various types are intermediate between normal subjects and extreme dichromats in the statistical sense of relative frequency. This problem can be decided only by painstaking study of large and random samples of various populations, and it is well-known to psychologists that the problems of obtaining representative random samples of populations are very considerable. Since it is already known that the frequencies of different kinds of colour vision defectives vary from one racial group and from one geographical group to another (Pickford, 1951), it is not satisfactory to assume until it has been proved, that in Sweden, Norway, Germany, France, Britain or the U.S.A., for example, or among different racial stocks in the same countries, the same types of defect will be found or that they will occur in the same proportions. Fortunately in colour vision we do not have to face the difficulty to be met in the study of the frequencies of left-handedness, for instance, which looks like a Mendelian recessive, that it is greatly modified in its manifestations by training and indirect environmental influences. We do, however, have to face what might be called the creation of types by the methods or weaknesses of tests and testing techniques. The lack of validity and reliability of the Ishihara and other tests, and the tendency of many workers to predict and even to assume types of defectives in accordance with one or other favourite theory, and to base their conclusions on too small and non-random samples of the populations tested, have caused many difficulties and confusions.

When the different types of so-called anomalous trichromats are studied in such a way as to group together those who are alike, it becomes clear that the colour different or truly anomalous form two statistically distinct groups (PA and DA), which would not become continuous, either with the normal population or with each other, however many subjects were tested (Pickford, 1951). Even if some intermediates between the normals, red anomalous and green anomalous, were found, there would never be enough of them to fill the gaps from the statistical point of view. The group of red anomalous or red different subjects (PA) is again subdivided in a statistically decisive way into those with and those without what is generally regarded as the special characteristic of protanopes, namely the darkening of the red end of the spectrum. There is, however, in respect of the distribution of greatly increased matching ranges, no very clear statistical distinction between the group of moderate protanopes or extreme protanomalous subjects (EPA) and the true protanopic dichromats, or between the group of moderate deuteranopes or extreme deuteranomalous subjects (EDA) and the true deuteranopic dichromats. The difference between deviant or anomalous deuteranopes and ordinary moderate deuteranopes looks as though it might be statistically significant if a large enough sample were tested. The distinction between all protans and all deutans, excluding the red anomalous without darkened red, is clear in terms of the shortening or darkening of the red end of the spectrum which occurs in the protanopic group, and there are no true intermediates of the kind created by the inefficiency of the Ishihara Test. The groups do not run together statistically.

For these reasons the distinctions between the colour different or truly anomalous and the whole groups of partial and complete dichromats of the types which have very large red-green matching ranges with or without big deviations, are clearer and more decisive than the distinctions between the moderate and complete protanopes or the moderate and complete deuteranopes, though the moderate deuteranopes must be divided into deviant and ordinary classes as explained above. It must be said that conclusions should be based on more extensive researches, using larger numbers of subjects chosen on a satisfactorily random and representative basis and also more tests all carried out on every individual. Willis & Farnsworth (1952) have made a valuable start in this direction, but they did not use a truly random sample. Unfortunately they have equated the mid-points and end-points, which are not physically or psycho-physically comparable, of six different types of anomaloscope test.

Table 2. *Frequencies of red-green major defectives in a group of* 142 *taken consecutively*

| | PA | | | | | | |
|---|---|---|---|---|---|---|---|
| | Red norm. | Red dark | EPA (Not subdivided here) | | | P | Totals |
| Protans | 4 | 10 | 14 | | | 16 | 44 |
| | | | EDA | | | | |
| | | | | Ordinary | | | |
| Deutans | DA | | Deviant | Less | Great | D | |
| | 52 | | 17 | 14 | 10 | 5 | 98 |

Table 2 shows the proportions of different types of red-green major defectives in a group mainly tested in Glasgow all of whom failed the Ishihara Test outright and were also tested with a form of anomaloscope using monochromatic gelatine colour filters (Pickford, 1951, with more material). They were taken in the order in which they were found, and, although a small number were relatives, they represent an approximately random sample. Nineteen were women: 1 PA with normal and 3 with darkened red; 1 EPA; 12 DA; and 3 EDA of the deviant type. Probably not more than 10 women at the most would be expected by chance in a group of 142 major defectives. Ten EDA subjects had mean deviations somewhat to the red side of the normal mid-matching point, and two EPA slightly to the green side. It will be observed that among protans the number of individuals increases in each type as the defects increase, whereas among deutans the number decreases. Although deutans are more than twice as frequent as protans, there are three times as many protanopic as deuteranopic dichromats.

## VII. Problems of heredity

Owing to the work of Waaler (1927) and others, it became clear that the defects of protanopes and deuteranopes were inherited by separate Mendelian recessive mechanisms, although both were totally sex-linked as realized before, and they are now regarded as dependent on genes having separate loci on the *X*-chromosome. Franceschetti (1928, 1949) added the so-called anomalous and extreme anomalous (PA and EPA, and DA and EDA), and pointed out that there must be two series of multiple allelomorphs, in each of which the gene for complete dichromasy was the most recessive, the normal gene the most dominant and the other genes were intermediate in order of the severity of the

defects they produced. Many researches by other workers have supported this theory and it has been widely accepted. Walls & Mathews (1952) have suggested that true intermediates between the two series may occur, as for instance, when a man has protanope and deuteranope genes together, but of course in different loci of his *X*-chromosome.

There are, however, in all probability further complexities, and there are certain pedigrees suggesting strongly that 'deviant deuteranopes' or 'anomalous deuteranopes', and red anomalous or protanomalous subjects with red of normal brightness are due to two additional allelomorphic genes, giving one normal and four rather than three defective genes in each series (Pickford, 1951). The fact that extreme protanomalous and extreme deuteranomalous subjects are very diverse and statistically indistinct from protanopic and deuteranopic dichromats respectively, makes it seem that there could be even more genes in action or that varying 'penetrance' and 'expressivity' could make marked differences to their effects. A further problem arises because there are certain defectives who are not clearly distinct from the extremes of the curves showing the distributions of normal deviations and matching ranges in the Rayleigh Equation, but whose defects are inherited true to type and, at least sometimes, definitely on the totally sex-linked pattern, excluding the normal heterozygotes for the familiar major defects. These are 'extreme deviants', and those who are exceptionally 'colour weak' in red and green, having fairly large matching ranges. Their defects are probably due to lesser defective genes, dominant to the genes for PA and DA but still recessive to the genes for normal colour vision in each series (Pickford, 1953*a*). It is likely, therefore, that in so far as the principal forms of red-green defect and their most marked minor forms are concerned, we must think of not less than five defective allelomorphs in each series, all, of course, totally sex-linked and some rather variable.

It is also very likely that, when women heterozygotes showing slight effects due to the familiar defective genes are excluded, there will be a number of variations of normal colour vision, inherited either by additive genes, or by non-additive genes partially sex-linked or not sex-linked at all, giving on the average equal proportions of defectives in the two sexes (Pickford, 1951). These observations apply also to yellow-blue defects (Pickford 1953*a*), though there may be more extreme forms of tritanopia inherited in other ways.

Various researches have suggested that major defects are incompletely recessive. Careful tests of normal heterozygotes show that there is a strong tendency for diminished forms of the types of major defect found among their male and female relatives to be present in them (Pickford, 1951). Normal heterozygotes of the red anomalous or protanomalous class, and of the green anomalous or deuteranomalous class, tend to show small red or green deviations accordingly. Protanopes, moderate protanopes and extreme protanomalous subjects, and deuteranopes, moderate deuteranopes and extreme deuteranomalous subjects, all tend to have normal heterozygote relatives who show large red-green thresholds or matching ranges. Deviant or anomalous deuteranopes tend to have normal heterozygote relatives who have marked green deviations in addition. The problem of the colour vision types of the abnormal heterozygotes, namely those who inherit two or more defective genes, is too complex to be discussed here, but PA may sometimes be only partially dominant to EPA (Pickford, 1953*b*).

The generalization may be made, for both major and minor forms of defect, and whether they are totally sex-linked or not, that the more that matching range is the main

feature, as with extreme anomalies and dichromasy, the more recessive the defect in the Mendelian sense, and the more that deviation is the main feature, the more dominant the defect.

## VIII. Modes of colour vision variation

There are three principal ways in which colour vision differences may be viewed from the sensory aspect, in comparison with normal colour vision. These modes of variation include small differences, classed as minor defects, as well as large differences or major defects, such as anomalous colour vision and colour blindness. The three modes of variation, which sometimes occur together, are as follows.

(1) Increases in the range of matching for the discrimination of red and green mixture with a yellow standard, and of a yellow and blue mixture with a colourless grey. Small increases lead to colour weakness; large increases to partial and eventually to complete colour blindness for the two colours in question. Yellow and blue defects will be omitted from any further discussion.

(2) Deviations from the normal mid-matching point in the Rayleigh Equation. Small deviations up to about three times sigma cause deviant colour vision. Big deviations, of about six or seven times sigma give colour differences or truly anomalous colour vision, and deviations to the limits of the equation on either side would cause dichromasy because then the standard yellow could be matched with both variables in all mixtures throughout the scale.

(3) Darkening of one of the hues called primaries, or shortening of the red or the violet end of the spectrum. In red-green blindness this shortening or darkening affects the red end of the spectrum and is found exclusively in protanopes and in the majority of protanomalous subjects. Shortening or darkening of the violet end of the spectrum is found in a proportion of yellow-blue defectives.

These three modes of variation at the sensory level are not necessarily equivalents of three different functions at the physiological levels (assuming that the sensory level is the level of conscious experience), but they are the end products of complex physiological processes which must be unravelled for the purposes of colour vision theories.

## IX. The trichromatic theory

On the trichromatic theory we should expect at least two kinds of colour vision defects: (1) those due to losses of receptors or perhaps of pathways ('loss' defects); (2) those due to lack or breakdown of the difference between two kinds of receptors or pathways without actual loss ('collapse' defects). Loss defects would lead towards deviations, colour difference and truly anomalous colour vision (PA and DA), and could, if extreme, produce dichromasy. It is not clear that they would produce increased matching ranges until the dichromatic point was reached. There would be a high correlation between degree of deviation and darkening of the part of the spectrum corresponding to the loss, and also with magnitude of defect apparent in daily life. However, the red end of the spectrum is not darkened in about half of the protanomalous subjects (PA), and it is apparently as much darkened in some of these and in extreme protanomalous subjects (EPA) as in many dichromatic protanopes. In other words, there is no correlation between magnitude of loss and darkening of red. This lends support to other evidence that brightness must be mediated at least in part by independent receptors, as suggested by Piéron (1952).

Since the brightness of red may vary independently of sensitivity to red, it is difficult to understand how brightness could be mediated mainly by a central relay system which was dependent on the integrity of the red receptors.

These facts also make us doubtful whether red difference or protanomaly can always be a loss defect. Extreme protanomaly or moderate protanopia would not be likely to be a pure loss defect either, because in it there is a great increase of the red-green matching range as well as a very variable degree of red deviation and loss of brightness of red. Complete protanopia, amounting to dichromasy, however, might be a pure loss defect on this theory, but it would be a useful experiment to find out whether the loss of brightness of the red end of the spectrum was the same in all dichromatic protanopes. On the other hand, the green different or strictly deuteranomalous (DA), who would be expected to be pure loss defectives because of their great deviations to the green side in the Rayleigh Equation, never show the loss of brightness of green which would be expected on the trichromatic theory. Since brightness of green is not reduced in extreme deuteranomalous subjects and dichromatic deuteranopes, it seems clear that they cannot be loss defectives, and this has been widely realized.

Collapse defects would lead towards increases of the red-green matching range, without darkening of either hue, and without changes or deviations of mid-points in the Rayleigh Equation, unless loss defects were combined with them, when the deviation would be towards the side of the loss in question. Then there would be a high correlation between magnitude of deviation and darkening of the appropriate colour. Hence it seems clear that the protanomalous with red of normal brightness can neither be pure collapse defective nor combine collapse and loss. Deuteranomalous subjects (DA) cannot be pure loss or collapse defectives, and, since moderate and deviant deuteranopes (EDA) all show variable degrees of green deviation uncorrelated with their increases of red-green matching range, it would appear that they cannot be pure collapse defectives either, but many of them, it seems, particularly the deviant or anomalous deuteranopes, might combine loss and collapse defects. This, however, is not possible, because none of them show darkening of green however great their green deviations, and a few, in the writer's experience, even have red deviations, just as a few protanomalous subjects have deviations of mid-points to the green side, combined with darkening of the red.

All these difficulties are felt in similar ways on a smaller scale in dealing with minor defectives, that is to say, those who have small increases of matching range and small deviations. It is unfortunate that such considerations shake our faith in the trichromatic theory very severely, and they led the writer to relinquish it altogether in the past, but it would be unscientific to be in any way dogmatic or to regard the matter as settled, though it does not help the trichromatic theory to say that the physiology of colour vision is so complex that anything might happen.

## X. Bipolar and zone theories

On a modified form of the ingenious hypothesis due to Houstoun (1932), following Hering's theory, it would appear that the physiological processes underlying the sensations of red and green might be controlled by a bipolar system. There is much evidence pointing in this direction from other sources, and many parallel considerations apply to yellow and blue which might also be controlled in a bipolar manner. On such a system the difference between deviation and matching range defects could be explained much

more efficiently and economically than on the trichromatic theory as usually understood. If yellow sensitivity did not vary independently of red and green it might be possible to think of a trichromatic theory in which red and green formed one bipolarity, and another bipolarity was formed between red and green on the one hand and blue on the other, after the fashion of Ladd-Franklin's scheme, but unfortunately this is not possible because yellow does vary independently.

On Houstoun's theory we may suppose that the red-green pair of receptors or of processes or functions have a point of change-over from the 'red' signal or response to the 'green' signal, whatever their physiological nature may be, and that the same may be true for yellow and blue, although of course, yellow is contributed partly by an additive process of red and green when we are using stimuli which do not correspond to complementary sensations. If all the red-green receptors or pairs of receptors or functions change over together and at the usual point, we have normal red-green discrimination and no deviation. If a number tend to change over at various points or irregularly, we tend to have increased red-green matching ranges, most extreme when all responses are in one direction alone, or half in each direction at the same time. When most of the responses tend to change over at a particular though exceptional point, we have deviation, colour difference or true anomaly. Loss defects would probably also have to be taken into account.

On this theory there would be an independent brightness receptor, though brightness is, as Hering supposed, contributed in part by hue responses as well. Major defectives excluded, it seems that losses of sensitivity to hue are correlated with decreases of brightness to a lesser extent for red and yellow than for blue and green (Pickford, 1951). Among protans (PA, EPA and P), however, loss of brightness of red is irregularly connected with losses of sensitivity to red in the ways described above.

Nobody will wish to deny that pigmentation of the transparent parts of the optical system may affect colour vision and brightness sensitivity and the relationship between them in certain ways, but it would be difficult to see how it could account for more than a small proportion of the variations reported. It is not impossible that a system such as that of Houstoun might be combined with a trichromatic system in some complex way such as that described by Talbot (1951), after Müller and Adams, or by Thomson (1952), after Müller, and brightness or whiteness might be controlled partly by a central relay system dependent on the hue receptors, but it is very unlikely to be completely dependent in this way. Some tests and experiments seem to stress the trichromatic nature of colour vision, while others emphasize its bipolar and tetrachromatic characteristics, but it is probably important for us to think that these need not be incompatible. It is probable, however, that the tetrachromatic theory of Houstoun would save a whole zone, on the zone theories of Müller, Adams, Talbot and Thomson, and this would be a considerable economy of hypothesis, though Nature does not necessarily work in the most economical way possible.

The form of Houstoun's theory suggested here would satisfy the requirements of the trichromatic aspects of colour vision, as Houstoun himself intended. It would give an adequate and easily intelligible account of all forms of red-green and yellow-blue defects and variations, and would make it clear, for example, how defects in yellow sensitivity are often connected with brightening of yellow, while blue defects are not necessarily accompanied by darkening of blue or violet. The theory would also explain the fact that

many of the subjects classed as extreme protanomalous or deuteranomalous defectives show a chracteristic fluctuating kind of sensitivity in red-green discrimination. They call the red in the Rayleigh Equation sometimes 'green' when it has enough red in it for them to distinguish it from the yellow, and sometimes 'red'. While they call the green sometimes 'red' and sometimes 'green'. They may feel confused themselves, and these responses might be due to a fluctuating variability of the balanced response mechanism. Sometimes most of the receptors may give either a 'red' or a 'green' signal to the red or to the green stimulus, but irregularly. Whereas in other defectives who have the same degree of defect the responses may be consistent, always the 'red' signal for the red stimulus, and always the 'green' signal for the green stimulus, when it has enough of the appropriate light in it to be discriminated from the yellow.

It seems that a bipolar theory, perhaps along the lines of the Hering-Houstoun theory, or of another kind, or possibly a zone theory in which one zone is concerned with the bipolar aspects of colour vision, is necessary in order to understand the facts of anomalous colour vision adequately.

## XI. Conclusions and summary

1. The concept of anomalous trichromasy should be reserved for several forms of major red-green colour vision defect, and other terms, namely 'deviant' and 'colour-weak' should be used of those who have minor defects in red and green (and also in yellow and blue).

2. The Rayleigh Equation is the most satisfactory simple test for anomalous colour vision, if used in such a way as to measure the subject's entire matching range, from which his mean deviation can be obtained. Brightness equations must always be obtained before colour equations are accepted.

3. The main types of anomaly are red anomaly or protanomaly, PA, with and without darkened red; green anomaly or deuteranomaly, DA; extreme protanomaly and deuteranomaly, EPA and EDA. The latter are divided into ordinary and deviant groups, and both are distinguished from PA and DA respectively by the magnitude of the matching range rather than the deviation, though this is usually smaller. There are also probably lesser forms of protanomaly and deuteranomaly which are difficult to distinguish from the extreme forms of normal red and green deviation, but belong to the sex-linked series.

4. The frequencies of these types are such that in a random sample of 142 major defectives, although protans were about half as frequent as deutans, protanopic dichromats, P, were three times as frequent as deuteranopic dichromats, D, and the DA group were more than three times as frequent as the PA group.

5. In heredity there are two series of about five multiple allelomorphs, all totally sex-linked, in which deviation defects tend to be dominant and matching range defects recessive.

6. Anomalous colour vision is dangerous in all situations in which life and safety depend on red, yellow or amber and green signals, and adequate tests must be used for measuring it.

7. Thorough empirical studies of all colour vision tests should also be undertaken using adequate statistical techniques and properly constituted random samples of populations.

8. A bipolar theory of colour vision, in one or another form, is required to account for the facts of anomalous colour vision, and also for minor variations of deviation and matching range.

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# A NOTE ON EYSENCK'S ARTICLE 'THE EFFECTS OF PSYCHOTHERAPY: AN EVALUATION'

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In *The Journal of Consulting Psychology*, Eysenck (1952) surveyed reports on improvement of neurotic patients after psychotherapy and found that '. . .the results compared [well] with the best available estimates of recovery without benefit of such therapy. The figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder. In view of the many difficulties attending such actuarial comparisons, no further conclusions could be derived from the data whose shortcomings highlight the necessity of properly planned and executed experimental studies into this important field.'

Dr Eysenck's 'experimental group' draws on the cases in twenty-four articles on the result of treatment by psychoanalytic and eclectic psychotherapeutic methods. The two sources of his 'control groups' are Landis's (1938) statistics on the consolidated amelioration rate of the state hospitals and Denker's (1946) survey of 500 consecutive disability insurance claims due to psychoneurosis treated by general practitioners. The main implicit argument runs that it is a matter of indifference whether psychotherapy is given or not since 'the figures' are indeterminate and so, it is explicitly stated, we should not insist on training clinical psychologists as psychotherapists! The latter point is a real *non sequitur*. If medical doctors had followed such advice their entire science would not have developed.

I am very much impressed with the negative results cited, based on the heavy array of statistics, and with the necessity for doing properly planned and executed research. Unfortunately, the defects of these studies are not presented fully or taken seriously enough. Some readers might jump to hasty conclusions that go beyond the principal fact: we do not have any studies which validly compare psychotherapeutic treatment results with the absence of such treatment.[1]

Specifically, I have two main criticisms and several minor ones. I do not believe Eysenck has an adequate control group nor that comparisons of groups can be made within the experimental group. Secondly, judgements of 'improvement' after treatment in these studies are very ambiguous.

To conclude as he does, Eysenck must assume patients do something they do not do: randomly self-select themselves to psychiatrists, general practitioners, and state hospitals. His 'treatment' versus 'non-treatment' groups are composed along very selective lines. Many of the 'insurance' group would probably never have visited the doctor if it were not required. As a whole the group is probably of higher social and economic level than other groups. Very likely the choice of a general practitioner rather than a psychiatrist to treat their psychoneurosis reflects a not-to-be-ignored difference in attitude to their illness. And, for all we know, different brands of psychiatrists may get self-selectively different samples of patients. Eysenck says we need better control groups, but fails to take his own statement seriously. Perhaps he is impressed by the size of his samples and forgets the lesson of the famous Literary Digest poll.

Pursuing his thesis, he also purports to show that different forms of treatment get

[1] Eysenck neglects the *one* outstanding exception, Rogers & associates (1949, 1950, 1951).

different results. Some evidence is available on the obvious differences in the initial state of the patients who are given different forms of psychotherapy. At The Menninger Foundation the Psychotherapy Research Project is developing a scale for rating mental health.[1] Health ratings on this scale by psychotherapists and supervisors of psychotherapy confirmed our impression that the patients who start psychoanalysis begin on the average at a higher level than those who are given other forms of psychotherapy. Those who are hospitalized begin much lower than those who are given out-patient treatment. It is very likely, therefore, that when Eysenck reports that 44% improve with psychoanalysis, 66% with eclectic treatment, and 72% with custodial treatment he is talking about groups of patients who are grossly different.

At the very least what is needed for an adequate control group is elementary matching of groups of patients on level of mental health, symptoms, and duration of illness. Both control and experimental groups also should need treatment equally, but the former should need it and not get it for a definite period. Even this additional statement, 'and not get it', represents the beginning of what might be serious incomparability. The fact that a person tried to get treatment and did not get it can have varying effects: for some people it can pull them together, for others it can worsen their state. The only study I know (Rogers & associates 1949, 1950, 1951) that used an 'own-control' method in which the patient is retested after a period of waiting, confirms the fact that such waiting makes a difference. Further requirements for an adequate control group are described in a recent article by Hunt (1952).

About the second main point: the terms 'recovered, improved, slightly improved, unimproved' are useless for Eysenck's purposes. The terms say nothing about what the patient was like at the beginning and end of treatment; they can be and are applied to patients at the entire range of mental health. A schizophrenic patient can be called 'recovered'; so can a patient with a slight personality problem. Obviously the word 'recovered' is used differently in each case. An absolute scale avoids this pitfall of conventional improvement scales. For instance, as compared with patients in expressive psychotherapy, psychoanalytic patients began at a higher level of mental health. One would expect therefore they would have less room for improvement. Health ratings by psychotherapists and their supervisors of the Menninger Clinic patients' initial and later state showed this to be true.

Another reason for the discrepancies in 'improvement' judgements is the varying point of view of the judges as to what constitutes improvement or recovery. For example, analysts may be more strict about the kind of personality change they consider recovery —or their patients are more strict about the standards they wish to apply to themselves. It is likely that general practitioners would consider it 'improvement' if the patient simply went back to work, while for the psychoanalytic and eclectic therapists the judgement of 'improvement' would probably be based on personality change. Probably most patients in psychoanalysis were working or were able to work up to treatment, and continued through treatment and thereafter.

A few other minor points:

(*a*) Some of the elements of psychotherapy (e.g. support and suggestion) are present in

[1] The scale consists of 100 points, where 100 represents ideal health and 0 total disintegration. Many intermediate points are defined and exemplified by thirty-two graded case descriptions. In making a rating the judge must balance off our seven criteria of health, consider the approximate position of his case within the sample case series, and then arrive at a single global rating. The scale is unpublished, but available to research personnel in mimeographed form.

a large number of the general medical practitioners' treatments. Also as Landis (1938) points out (in objecting himself to the use of the consolidated amelioration rate as a baseline for 'spontaneous' recovery) neurotics in state hospitals are given a variety of treatments, including some psychotherapy. And, as they are relatively unusual occupants of state hospitals, they probably get unusual treatment. At least, it is not entirely true that Eysenck was comparing a no-treatment with a treatment group.

(*b*) Eysenck includes under the category 'not improved' also 'died'. These should be taken out of his samples. Death, obviously, is not on any improvement-unimprovement continuum. Furthermore, the deaths in the samples are inconsistently included.

(*c*) Under the psychoanalytically treated group, Eysenck includes reports of results of treatment done from 1920 to 1930 in Fenichel's study (1930).[1] These data are older than others reported, the large number of cases over-weights the others in the subgroup, and a brief can be made that theory and technique have improved since those days. (But try and prove that one!)

In general, Eysenck is much impressed by the ease with which one's subjective feelings can be wrong. He quotes with approval the findings of the Michigan Project on the Selection of Clinical Psychologists (Kelly & Fiske, 1950) to the effect that the greater the *certainty* the poorer the *accuracy* of clinical judgement.

This was often but *not* consistently true—clinicians were sometimes both certain and accurate. (See later report, Kelly & Fiske, 1951.)

There are many difficulties in psychotherapy research. Clouding the facts and coming to wrong conclusions increases these difficulties. Clinical psychologists who have training and understanding in the psychotherapeutic process are in a good position to attack many tough problems involved in psychotherapy research; those who have withdrawn cannot help us to understand.

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[1] One member of the Psychotherapy Research Group, Dr Hellmuth Kaiser, formerly worked on the staff of the Berlin Psychoanalytic Institute from which Fenichel's report came. He recalls that many of the patients were treated in the Polyclinic by analysts who were treating their first cases. The judgements of improvement were made without any attempt at follow-up. Furthermore, the cases were screened for treatment very casually; many were completely unsuitable for psychoanalysis. The therapists' ratings may be too favourable or too unfavourable to psychoanalysis—their ratings help little in knowing the true state of affairs.

(*Manuscript received* 22 *October* 1953)

# A REPLY TO LUBORSKY'S NOTE

By H. J. EYSENCK

*Institute of Psychiatry, University of London*

Luborsky appears to have two objections to my paper on 'The effects of psychotherapy'. The first one is that the conclusions do not follow from the data; the second one is that 'the main implicit argument', based on these conclusions, is a *non sequitur*. He seems to feel strongly on these points as he appears to accuse me of 'clouding the facts and coming to wrong conclusions'.

I find it difficult to answer his paper as I agree with most of what he says throughout. He is concerned with pointing out the shortcomings of the researches I have summarized, both from the point of view of selection of control and experimental groups, and from the point of view of the clinical judgements made of the improvement or otherwise of the patients. With these criticisms I agree; I have myself commented, in the article criticized, on the poor quality of the data 'whose shortcomings highlight the necessity of properly planned and executed experimental studies into this important field'. I am grateful to Luborsky for dotting the i's and crossing the t's, but I fail to see how this amounts in any sense to a criticism since it supports what I myself have written.

Basically, I think, Luborsky has misunderstood the logic of my approach. He seems to imagine that I tried to prove from the existing data that psychotherapy has no effect; this would be foolish (because you cannot prove a negative) as well as inadmissible (because of the poor quality of the evidence). What I tried to do, however, was something quite different: I tried to show that the available evidence, in spite of, or because of, its poor quality, fails to support the hypothesis that psychotherapy has any beneficial effects. I did not try to prove the null hypothesis, but merely examined the efforts of other investigators to disprove it. My conclusion was, and Luborsky does not attempt to deny its correctness, that these efforts ended in failure. As I point out in my conclusion: 'The figures fail to support the hypothesis that psychotherapy facilitates recovery from neurotic disorder. In view of the many difficulties attending such actuarial comparisons, no further conclusions could be derived.'

I do not see that Luborsky in any way disagrees with this statement; if he has found data to support the hypothesis of psychotherapeutic usefulness he certainly fails to mention them. He does say explicitly 'we do not have any studies which validly compare psychotherapeutic treatment results with the absence of such treatment'. I take this to be in complete agreement with my own conclusion that there is no evidence to show that psychotherapeutic treatment is successful. (Luborsky says in a footnote to the above sentence, 'Eysenck neglects the *one* outstanding exception'. This exception turns out to be a set of mimeographed interim reports of an experiment which, when it is completed and published, will undoubtedly be relevant to the problem under consideration.)

With regard to Luborsky's first criticism, then, we must conclude that it rests on a misunderstanding. Whether we are willing to accept the researches reviewed by me, bad as they are, as being of interest, or whether we reject them all, as Luborsky would seem to wish us do, we come to the same conclusion, namely, that *there is no scientific evidence*

*regarding the beneficial effects of psychotherapy.* This is the point I wished to make and this is the point which Luborsky apparently agrees with.

What consequences follow from this demonstration? The views expressed in my paper were these: 'Until such facts as may be discovered in a process of rigorous analysis support the prevalent belief in the therapeutic effectiveness of psychological treatment, it seems premature to insist on the inclusion of training in such treatment in the curriculum of the clinical psychologist.'

Luborsky regards this as a *non sequitur*, i.e. he does not feel that this conclusion follows logically from the premises. I may have been at fault in not stating my major premise and not putting down in detail the intermediate steps of the argument. The main reason for this neglect was that they seemed to me self-evident; apparently I was wrong in this assumption. My main premise, then, is that psychology is a science; in other words, that it deals with observable facts and verifiable theories and that that which is not observed or verified does not form part of psychology. A clinical psychologist on this view would be a psychologist who applies approved scientific methods in the abnormal field and uses his scientific knowledge in dealing with the problems that arise. To apply methods having no proper rationale and to use treatments, the hypothesis of whose effectiveness is not supported by any empirical studies, does not appear to be the proper exercise of his scientific training, and it would seem to follow that such training should not include therapeutic procedures until their effectiveness had been proved beyond reasonable doubt. Lehner's comment seems apposite here, referring to 'therapy as an undefined technique which is applied to unspecified problems with non-predictable outcome. For this technique we recommend rigorous training.'

Luborsky attempts to gain sympathy for his plea by saying that 'if medical doctors had followed such advice their entire science would not have developed'. This statement ill agrees with my reading of medical history: I cannot recall any reputable method of treatment in modern times, used as widely as psychotherapy, and advocated as enthusiastically, which has been in use for over fifty years without any serious attempt to prove its effectiveness. This fact in itself is an interesting social phenomenon on which much could be said.

In summary, it appears that Luborsky has misunderstood the logic of my analysis, and that he does not, in fact, disagree with my conclusions. His disagreement with deductions made from these conclusions may be based on genuine differences of opinion, or on the fact that the major premise of my argument was not explicitly stated. If the latter, then it may be assumed that we are now in agreement; if the former, then we disagree in the sense that I regard psychology as a scientific discipline, while Luborsky does not. On this issue the reader no doubt will form his own opinion.

(*Manuscript received* 5 *December* 1953)

# CORRESPONDENCE

The Editor
*British Journal of Psychology*

Dear Sir,

Recently, M. M. Lewis presented a critical evaluation of our monograph on *The Acquisition of Word Meanings*.[1] The present note is intended to correct misapprehensions which may have been occasioned by this criticism. For this purpose, it will be necessary to sketch briefly the aim of the study, the nature of the method employed, the rationale underlying the testing of children of different ages, and the sorts of analyses undertaken.

The problem which we posed for ourselves—a problem grossly neglected by psychologists, experimental and otherwise—was the determination of the nature and variety of semantic processes as a function of developmental level. To this end, a special methodology was devised and children ranging in age from 8 years 6 months to 13 years 5 months were tested.

The method involved the establishment of a miniature model of the sort of situation confronting anyone who might seek to arrive at the meaning of an unknown word appearing in a variety of verbal contexts. The technique utilized artificial words, with six sentence contexts for each word; the subject's task was to give meaning to the artificial word on the basis of the clues offered by the various contexts. More precisely, the subject had to give the meaning of the artificial word, first in terms of the initial sentence alone, then in terms of the first two sentences, then in terms of the first three, and so on. With each addition of a new verbal context, the subject could maintain or change his solution; in either case, he was obliged to give his reasons for so doing, and was also required to show how the solution fit all of the contexts previously presented.

In the main, the series were so constructed that a single word, taken literally, could fit all six contexts. For several of the series, however, the English language did not provide a single word which could be taken literally for all six sentences. For example, the series containing the artificial word 'soldeve' consists of the following six sentences: (1) The dinner was good but the fruit we ate was soldeve. (2) When we were driving in the evening we did not feel safe because things on the road seemed to soldeve. (3) The older you get the sooner you will begin to soldeve. (4) People like a blossoming plant better than one that is soldeve. (5) Putting the dress on the sunny lawn made the colour soldeve. (6) Because the windshield was frozen things looked soldeve. An adequate solution for this series would be 'fade' or 'wither'. A solution in these 'unorthodox' cases had to be somewhat stretched if it were to fit all of the verbal contexts in a series. For such series, as for all of the other series, the correctness of a solution was operationally determined by the performance of educated subjects. In these 'unorthodox' series as in the other ones, there was a high degree of agreement among college students as to final solutions.

This point is stressed because some criticism has been directed against the entire test on the grounds that several of the series permit no adequate solution. Such criticism might be relevant if it were suggested that the analysis were directed toward such questions as the point within a series at which a correct guess is made or, indeed, whether

[1] *The Acquisition of Word Meanings: a Developmental Study.* Reviewed in *Brit. J. Psychol.* XLIV, p. 269.

a correct guess is made at all for any particular context. These questions were not involved in the study. In so far as correctness entered at all—and it must be emphasized that correctness of solution was merely a secondary aspect of the analysis—it was in terms of the degree of approximation toward a conventionalized solution as defined by a consensus of educated adult subjects. In any case, the criticism, which seems to assume that the main concern of the experiment was with correct guesses, misses the major aspects of the investigation.

The experiment was chiefly directed at determining the processes involved in arriving at a solution. It was clearly evident that the same sorts of processes were utilized by children of a particular developmental stage in arriving at a solution for the 'unorthodox' series as were utilized in achieving solutions for the more orthodox series.

Now, with regard to the rationale underlying the employment of subjects differing in chronological age. Since many workers in the field of educational psychology are interested in establishing norms for specific chronological ages, it is important to stress that age was utilized in our experiment not in an absolute chronological sense, but rather as a relative index of development. In this respect, our approach bears a very close relationship to the manner in which Piaget utilizes age as an index of development. In other words, we used age with the assumption that the processes utilized by younger children to cope with cognitive problems are as a rule developmentally less mature than the processes utilized by older children. Whether certain processes are brought into play at a particular age will depend of course on the nature of the problem, the amount of experience of the subjects with the task, etc. Assuming these to be constant for all of the subjects, one can determine then what processes do emerge at various developmental levels to cope with the same experimental situation.

With regard to the analysis, we were predominantly interested in the nature and variety of the processes involved in giving meaning to words—not in the meanings *per se*, i.e. we sought to determine how children of different developmental stages handled the artificial word for each and every one of the six contexts comprising a series.

To give some idea of the findings of the study, only one aspect of one of the areas treated in the study may be mentioned, viz. that concerned with the nature and variety of the processes of signification. In the analysis, some fifteen different processes of signification were distinguished and ordered in a genetic progression. Statistical treatment revealed clear-cut evidence of a changing relationship, as a function of developmental level, between word-meaning and contextual involvement: for the youngest children, words and their sentence contexts were typically undifferentiated as to meaning—the word meant part or whole of the context in which it was embedded; with genetic progression, the word became increasingly detached from any particular context and finally reached the stage of a truly generalized concept. This and other findings are presented in considerable detail in the monograph.

In conclusion, we are convinced that the study does provide considerable information concerning the nature and variety of semantic processes in children, and also affords grounds for ordering these processes in a rational genetic sequence. The sort of approach we have utilized and the results obtained have been and are being fruitfully exploited in a series of experimental studies on the semantic activity of educated adults and uneducated adults, schizophrenics and feeble-minded children (Kaplan, 1950; Baker, 1953) The findings have also afforded an experimentally grounded basis for the interpretation

of psychological activity involved in semantic changes in the history of the living language (Werner, 1954).

Yours faithfully,

HEINZ WERNER and EDITH KAPLAN

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THE EDITOR
*British Journal of Psychology*

Dear Sir,

I welcome the opportunity of replying to this letter, as I was unable, in the space suggested by the Editor for my review, to substantiate my criticism as fully as I had wished.

I must repeat, what is made clear in my review, that I regard the general intention and the general planning of the investigation as fully satisfactory. There is work here to be done, since there are great gaps to be filled in our knowledge of the semantic development of children. It is all the more to be regretted that the authors should have given so much time to an investigation, the foundations of which are so insecure as to render any conclusions suspect.

Everything in this investigation depends, it is clear, upon the nature of the test-situations with which the children are presented. There are certain conditions which need to be satisfied; these are stated by the authors themselves in terms which seem to me unexceptionable. They say:

> The present study endeavors to investigate experimentally the processes underlying the acquisition of word meaning through verbal contexts. For this purpose the 'Word-Context Test' was designed. This test employs artificial words embedded in sentences. The subject going from one context to another is expected to arrive finally at the meaning of the word. Each of these artificial words signifies either an object or an action varying in degrees of concreteness. There are twelve series of six sentences each. The sentences in each series are, in general, ordered in such a way that, as a child moves from one sentence to the next, the clues increase in definiteness. (p. 1.)

In describing the administration of the tests, they say:

> The child was informed that he would be presented with twelve words which he never heard before; that these words were used in a little town out West and were not spoken anywhere else. It was made clear to him that each word had only one meaning throughout the six sentences. (p. 7.)

It is most unfortunate—and the justification of my criticism—that these conditions were not sufficiently satisfied in the devising of the test-situations. The intention was to present each unfamiliar word embedded in six English sentences; obviously it is essential, first, that there should be, as the authors say, 'one meaning throughout the six sentences'; or, as I would put it, at least semantic continuity; and, secondly, that since these are

English sentences presented to English-speaking children, there should at least be the possibility of the normal English range in the grammatical functions of the unfamiliar word from sentence to sentence. If, as is here the case, these conditions are not satisfied, there is the greatest likelihood that the children's responses will be determined not only by the manner in which they respond to unfamiliar words in meaningful contexts, but also by an immeasurable factor: the semantic and grammatical incompatibilities of the unfamiliar word from sentence to sentence. And the more literate the children are, the more likely are they to be confused by these incompatibilities.

To illustrate these points, I would draw attention to the *soldeve* example, cited in my review, and given in the letter above. The authors now call this an 'unorthodox example' but—as far as I can tell—there is no reference to its unorthodoxy in the monograph. I would also instance the following examples:

*Hudray*

1. If you eat well and sleep well you will hudray.
2. Mrs Smith wanted to hudray her family.
3. Jane had to hudray the cloth so that the dress would fit Mary.
4. You hudray what you know by reading and studying.
5. To hudray the number of children in the class there must be enough chairs.
6. You must have enough space in the bookcase to hudray your library.

*Contavish*

1. You can't fill anything with a contavish.
2. The more you take out of a contavish the larger it gets.
3. Before the house is finished the walls must have contavishes.
4. You can't feel or touch a contavish.
5. A bottle has only one contavish.
6. John fell into a contavish in the road.

My contention is that with test-situations as badly devised as these, no analysis of the results, however detailed, can provide trustworthy information about semantic processes in children.

Yours faithfully,

M. M. Lewis

Institute of Education
The University
Nottingham

# PUBLICATIONS RECENTLY RECEIVED

*Social Psychology.* By S. A. Asch. New York: Prentice-Hall Inc. 1952. Pp. xvi+646.

*Social Psychology.* By Theodore M. Newcomb. London: Tavistock Publications. 1952. Pp. xi+690. 30*s.* net.

*Social Psychology.* By W. J. H. Sprott. London: Methuen. 1952. Pp. xiv+268. 20*s.* net.

It is a commentary on the state of the subject that a reviewer can have before him three books with identical titles but remarkably different content. If, which is nowadays almost unthinkable, three books entitled simply 'physics' or 'chemistry' were produced, one could be reasonably certain that they would all contain approximately the same material; or that if one of them differed markedly from the rest, it would either be bad or else a work of Einsteinian genius. But the three books under consideration are all, in their particular ways, good books. There is certainly something to be learned from each, but although they have their areas of overlap, they all teach something different. Social psychology, in fact, has not yet established its basic premises. Its controversies do not arise from indecision as to the next step to be taken from an established base (as was the case, for instance, in the controversy over the wave and the particle explanations of light), but from a fundamental indecision as to the nature of the discipline, the appropriate methodologies, and the whole conceptual scheme by which relationships of man to his environment may be approached. Writers who do not understand the, as yet, rudimentary nature of our social-psychological concepts and techniques, produce naïvely all-conclusive theories about human nature and society. But Profs. Sprott, Newcomb and Asch are well aware that we are only at the beginning of things, and it is this realistic humility, coupled with a sensitive appreciation of the work already carried out, which makes their writing of value. It is perhaps a backhanded compliment to say that their books will help subsequent social psychologists to render them out of date before many years have passed.

Prof. Sprott's volume is the slightest of the three, but for this reason may prove to be the most satisfactory introduction. Moreover, it has the merit of being written by a man who has no psychological axe to grind. His erudition as a philosopher and a sociologist has given Prof. Sprott an admirable detachment in criticizing the theories of others. There are, it is true, certain gaps—for example, little is said of social role, which is perhaps the current key concept of social psychology—but on the whole the field is covered with lucid impartiality. Profs. Newcomb and Asch in their far more massive tomes (each has over 600 large pages, a deterrent almost as formidable as the cost) also cover the field of social psychology with great amplitude, but they do so to demonstrate a particular approach: Prof. Asch's is a gestalt approach, while Prof. Newcomb's combines the insights of the neo-Freudian with those of Lewinian field theory, though it must be admitted that these meagre descriptions do scant justice to attitudes towards human affairs which are elaborated with such scholarship and penetration. Of the two, Prof. Asch's is rather the more coherent, but he has perhaps taken the easier path. But both works will be indispensable to the student whose interests, it may be, have been stirred by Prof. Sprott's more easily digested work.

But while praising the work of men so able, and while recognizing how much they have done to systematize (albeit in somewhat varied systems) our knowledge of human motives and behaviour, it must be admitted that there are many central problems which remain untouched. The main one is, perhaps, that being human we can never attain real detachment and must always view man's actions through a screen of values and preconceptions for which no scientific assessment is possible, since they are outside the range of science. The intransigeance of this difficulty is one which becomes very clear if one compares work carried out in countries in which prevail differing attitudes towards, say, the meaning of responsibility, love, sex and duty. It is, for example, perhaps within the American ethos to *believe* more than we do in the value of the social sciences, while this in turn may spring from a sanguine belief in human perfectibility, which is typical of a new and vigorous civilization. For the psychologist whose work lies mainly in the laboratory, these difficulties are less intrusive, but so soon as we touch upon social behaviour, the problems of our work reflect in part our own problems, but often in so subtle a fashion that we have the illusion of complete objectivity.

But the works under review go a long way towards establishing a base from which more intractable difficulties may be assaulted.

ADAM CURLE

*Field Theory in Social Science.* By KURT LEWIN. Edited by Dorwin Cartwright. London: Tavistock Publications Ltd. 1952. Pp. xx+346. 21*s*. net.

This posthumous collection of theoretical papers written by Lewin during the last ten years of his life is a companion volume to *Resolving Social Conflicts*, in which his chief concern was with practical problems in society. The papers are selected and edited by the present Director of Lewin's Research Center for Group Dynamics, who assures us from that vantage point that 'they add up to a remarkably comprehensive statement of his major contributions'. The book begins with three essays in the philosophy of science. These three chapters, together with a technical appendix, are a declaration of Lewin's theoretical faith and a detailed—often exhilarating— exposition of its articles. The papers forming the other three-quarters of the book show how these guiding principles apply to a wide variety of specific problems in social psychology and neighbouring fields. The last chapter, reprinted from Carmichael's *Manual of Child Psychology*, summarizes the main theoretical and empirical findings of research carried out under Lewin's supervision.

Had Lewin written a book with field theory in social science as his theme it would certainly have been very much like this assembly of papers, but better. He would undoubtedly have developed his argument with a continuity, fullness and coherence which one has really no right to expect in a collection of separately conceived discussions, but the 'out-group' reader of this one may become a little daunted as he moves on and finds it repetitious, rather disjointed and occasionally over-compressed. However that is after all a minor matter of organization—unavoidable probably in this instance but odd, nevertheless, in a publication presenting the work of someone who so persuasively emphasized how important for a whole is the interdependence of its parts! In style and substance the individual papers are authentic and essential Lewin. As such they display his distinctive characteristics in all their strength and weakness, and their value is enhanced by the fact that all were written after the publication of the three books which have made his name in the English-speaking world. With so much first-hand and up-to-date material at his disposal the reader will feel better equipped to appreciate Lewin's theoretical contributions to social science, and any chronic sufferer from 'fact-happy' empiricism could hardly find a better antidote than these wide-ranging writings of a dedicated theorist with a flair for tackling practical problems.

A. J. FLOOK

*Instincts of the Herd in Peace and War.* By WILFRED TROTTER. Edited by R. W. CHAPMAN, with a foreword by F. R. M. WALSHE. Oxford University Press. 1953. Pp. xviii+220. 25*s*. net.

It shows the remarkable vitality of Trotter's work that this library edition of his book (prepared by R. W. Chapman, with minor corrections from Trotter's manuscripts and typescripts, and with a new and more comprehensive index) should be published while an ordinary edition is still in print. The book forms the best kind of memorial to the author, and Dr Walshe's Foreword, giving some account of Trotter's personality, shows him to have been one of the old civilized men of science, with a mind capable of interesting educated people. Dr Walshe regrets that although sympathetic towards religious experience and alive to its importance, Trotter viewed it detachedly and without having a religious faith himself.

Trotter's technical formulation of his insights is not acceptable to contemporary psychologists and could undoubtedly be improved. But he *has* insights, of which there are none too many in psychology, and he sees the relevance of his insights to the practical affair of living a civilized life. The astringent detachment of his writing is well illustrated in his remark: 'To suppose that when one has admitted the liability to prejudice one can free oneself from it by a direct voluntary effort is a common belief and an entirely fallacious one. Such a task is far beyond the powers of the most fully instructed mind, and is not likely to be undertaken except by those who have least chance of success'. Reading to-day, we have to make great allowances for Trotter's own wartime prejudices and for the pseudo-biological form in which they were expressed; he achieves much more detachment in the interesting and prophetic 'Postscript of 1919'. There are also obvious dangers in his conception of socialized gregariousness on the ant or bee pattern as an ideal for human communities. But to make his mistakes and dangerous tendencies an excuse for avoiding the stimulating challenge of the book would be to betray oneself as one of the 'stable-minded' to whom Trotter attributes responsibility for our worst social mishaps, including war.

D. W. H.

*Solitude and Privacy. A Study of Social Isolation, its Causes and Therapy.* By PAUL HALMOS. With a foreword by T. H. MARSHALL. London: Routledge and Kegan Paul Ltd. 1952. Pp. xvii+181. 21*s.* net.

The title of this book is deceptively modest. Dr Halmos does not merely discuss the phenomenon of 'social isolation' in terms of some standard psychological theory. He is concerned rather to put forward a new fundamental theoretical principle, namely that psychology needs to recognize the existence in man of a definite need for 'social participation' or fellowship, not merely as a secondary manifestation of the sex-drive, as in Freudian theory, but as a primary drive in its own right, rooted in man's biology as being an evolutionary factor with considerable survival-value. The recognition of such a '*principium sociale*' operating alongside the '*principium sexuale*' (the rather irritating jargon is one of this book's less fortunate features) would clear up, Dr Halmos claims, a number of difficulties in the Freudian and neo-Freudian theories, and he points out that something very like it has been postulated in different ways by such diverse psychologists as Rank, Fromm, Sullivan, Adler and Jung. In terms of it an interesting diagnosis of some of our present discontents becomes possible, for we live in a 'desocialized culture', where opportunities for social participation such as were afforded in past centuries by choral dancing and communal drinking have declined (statistics are provided to prove this) and an 'ideology of privacy and reserve' has grown up to confirm people in their separateness (passages are quoted from many 'representative' modern writers to illustrate this). While agreeing that in particular cases inability to 'participate' in social life will be a result of neurosis as ordinarily understood, Dr Halmos follows American psychologists of the type of Mead in holding that in the final analysis 'socio-cultural determinants precede and control individual-genetic development', so that desocialized culture must be regarded as one of the primary causes of individual neurotic development itself. He therefore concludes that individual psychotherapy will have only very limited success unless it takes place within the context of a much wider 'community therapy', by which he means not social reform of the ordinary kind, but the deliberate attempt to create fellowship-groups into which people can enter and find satisfaction.

There is little doubt that Dr Halmos is getting at something extremely important, but his argument begs some large questions, even for those readers who would be willing to agree that he has made his case for some sort of need for relationship in human beings over and above secondary manifestations of sexuality. The principal question is one of phenomenology and logic. He uses the term 'friendship', or his own term 'biosocial participation', indiscriminately to cover all group-relationships except mechanical regimentation, and seems unaware that there are differences between groups so great as to make nonsense of this. Can the loss of personality in herd-relationship, such as comes from the tribal dance or alcoholic intoxication, really be said to satisfy the same need as friendship in which personality is enhanced? Or, to put the point in another way, does not the demand for privacy itself express a fundamental need of man, the need to escape from the 'dear octopus' of the herd in order to become fully human? Is not privacy indeed the precondition of real friendship? It is to be hoped that Dr Halmos's book will stimulate further reflexion and research on the important question of the *differentia* of relationships between persons, without which community-therapy is liable to be a groping in the dark. JOHN WREN-LEWIS

*Sigmund Freud. Life and Work.* Vol. I. *The Young Freud, 1856–1900.* By ERNEST JONES. London: The Hogarth Press. 1953. Pp. xiv+454, with 10 illustrations. 27*s.* 6*d.* net.

This, the first, volume of a biography which will doubtless rank as classic in the eyes of posterity conveys the impression of being above all a labour of love on the part of the author; but it is conceived in no spirit of hero-worship. It is conceived rather in a spirit of admiration tempered by the most realistic appraisement of the emotional factors influencing Freud's intellectual achievements. In view of the outstanding nature of these achievements, the appearance of the present record is particularly timely; for, since Ernest Jones is the last remaining of the inner circle of Freud's collaborators, there is no other who could ever speak with the authority which he commands. What has influenced him most in the preparation of the biography is 'the hope of elucidating something of the processes in Freud's mind, and the experiences in his life, that culminated in his discoveries'. In a sense, therefore, the biography may be regarded as representing a psychoanalytical study of the origin of psychoanalysis. One of the most important sources drawn upon for this study is the remarkable collection of intimate letters written by Freud to his friend Wilhelm Fliess between 1887 and 1902—a period covering the most original phase in Freud's thought; and, as Jones convincingly shows, there was an interreaction between Freud's intellectual productivity at this time, the psychoneurosis from which he is now known to have suffered, and his emotional relationship with Fliess. The picture of Freud which emerges, so far from conforming to

the popular picture of the detached observer calmly elaborating theories about human nature, is that of a sensitive soul heroically struggling to solve his own emotional problems, and in so doing to achieve scientific understanding of the mainsprings of human motivation. It was in the course of this struggle that Freud was inspired to embark upon the arduous task of self-analysis, which resulted not only in his achieving the remarkable personal adjustment which characterized his later years, but also in the conceptualization of his insights in the psychoanalytical theory which has made his name famous. The enormous labour undertaken by Jones in exploring the sources of the intellectual and emotional influences which bore upon Freud, and in relating these to the theoretical system of psychoanalysis which emerged under the influence of Freud's genius must be regarded as amply rewarded; and the reader of the first volume will look forward to subsequent volumes with eager anticipation.

W. RONALD D. FAIRBAIRN

*On Aphasia.* By S. Freud. Translated by E. Stengel. London: Imago Publishing Co. Ltd. 1953. Pp. xvi+105. 12*s.* 6*d.* net.

Dr Stengel has performed a most valuable service in translating this fascinating little early work of Freud. Long since rare in German, and never before translated into English, it now takes its place as an important link in the history alike of psychoanalysis, and of the study of disordered language function. That Freud drew some inspiration from Hughlings Jackson must have suggested itself to everybody who has seriously puzzled over the idea of 'regression' on the one hand, and that of 'dissolution of function' on the other. In this book Freud explicitly bases his constructive views about aphasia upon those of Jackson. In doing so he was a pioneer—preceded, it seems, only by Arnold Pick. The interest of these two appears to have found no response, until Henry Head republished Jackson's papers in 1915. It is an intriguing fact that Head, although he mentions Freud as the inventor of the term *agnosia*, gives no evidence that he had ever read Freud's book, yet this, alone of previous works—with the exception of Pick's papers—propounded the need for a functional and psychological, as opposed to an anatomical, interpretation of the phenomena of aphasia along lines directly suggested by Jackson, and so brilliantly developed by Head himself.

That these ideas, derived from Jackson in connexion with aphasia in particular, and the dissolution of nervous functions in general, exerted a profound influence upon the later development of Freud's thought is evident. Freud characterized his psychoanalytic theories as 'superstructure which will have to be set on its organic foundation at some time or other'. Jackson did not hesitate to extend formulations rooted in neurological fact into the field of psychological observation. The lines of thought which they respectively developed are perhaps among the most significant that offer hope for the future. It is pleasing to learn from this book that these two men were not mere contemporaries. R. C. O.

*Ego Psychology and the Psychoses.* By Paul Federn. Edited and with an Introduction by Edoardo Weiss. London: Imago Publishing Co. Ltd. 1953. Pp. 375. 30*s.* net.

Paul Federn trained under Freud and was closely associated with him from 1903 onwards until he left Vienna and settled in New York in 1938. He was one of the original Freudian circle of eight. He died in 1950 at the age of 79.

The book is divided into three parts: (1) on ego psychology; (2) on the treatment of psychoses; (3) two papers on narcissism. There is an excellent introduction by Weiss, a bibliography of Federn's publications and an index. Most of the chapters are reprinted from Federn's published writings, but a few are from manuscript or compiled from his papers. They date from 1926 to 1949. As one would expect, they are varied in length and a great deal of repetition was inevitable. It would have been an advantage if the dates of the various publications had been given in the list of contents. If these dates are looked up in the footnotes at the beginnings of the chapters, or in the list of Federn's works, it is found that the chapters in each of the three sections of the book are in order of publication, except those now published for the first time.

Part two of the book, on the treatment of the psychoses, is the most readable and easy to understand, and will be of special interest to psychiatrists and psychotherapists who are dealing with the psychoses. Federn had much experience with the problems of the treatment of the psychoses. In 1906 he started experimenting on the therapy of what were regarded as hopeless cases, knowing that he could not do them any harm, and gradually worked out therapeutic techniques for dealing with them, especially for schizophrenia. He shows that Freud was right in saying that psychotic states cannot be treated by psychoanalysis only in the sense of the classical type of analysis, which was formed to suit neurotics, but that it can be modified in such a way that with the insight given by ego psychology it can also be used

therapeutically for psychotics. Many people are now working on the problems of the psychoanalytic therapy of psychotics, and therefore the book will excite considerable interest and be of much value.

The introduction by Weiss is very helpful, and it is rationally correct that the book should start with a theoretical part which explains the ego psychology with which Federn works. In practice, however, the presentation of this part, the ego psychology and its implications, is very abstract and will be difficult for the reader, and it would have been better to start with the second part of the book, which is more concrete and is presented in such a way that the reader can see why the special theories about psychotic cases should have been formulated. It is more natural to step from problem to theory and then to practice, than directly from theory to practice. In any case the book inevitably suffers from the mode of its construction out of a series of papers written separately for various occasions and purposes, and it would have been very much more satisfactory if it could have been wholly rewritten. Then the various steps of theory could have been integrated with concrete material and examples showing how psychoses, latent and partial psychoses and pre-psychotic states were handled in the therapeutic situation. If this had been done, the theories relating to ego psychology could have been inter-related with other branches of psychoanalytic theory, such as the work of Melanie Klein, which Federn considers of special importance but never discusses.

Federn points out that every psychosis is a mental disease of the ego itself, while in neurosis only some functions of the ego are impaired. In his handling of the psychoses his practices are not difficult to grasp, and are closely related to methods which have occurred to many of us independently in our approaches to partial and latent psychotics and to the psychotic aspects of some disturbances which would be classed as neurotic in conventional diagnosis. Federn appreciates this, but claims that his ego psychology is a direct derivative from the truly Freudian plan, and that it will enable us to treat these problems with clear scientific insight and knowledge of what is being done to the patient, rather than by methods which though often more successful than classical psychoanalysis for certain patients, are essentially unscientific or indirectly derived from the Freudian line.

His formulations of the processes involved in psychotic disturbances are brilliant and fascinating, and they show him to be a man of exceptional insight. The applications of the ego psychology itself, however, are not always quite convincing. This is because he usually leaves out the analysis of the deeper unconscious mechanisms at work in his efforts to concentrate attention on the all-important ego-aspects which are neglected by most other writers.

In general he explains that schizophrenia and schizoid states arise from breakdown and inadequate libido cathexis of the inner boundaries of the ego, so that it is invaded by deeply repressed material and impulses and also loses its firmness and coherence. Depressive states are in general due to the cathexis of the ego boundaries with mortido or the death instinct. In the psychoses there is a tendency for ego boundaries to be withdrawn and to regress to earlier and more infantile forms. In this way in paranoid states thoughts are sometimes excluded from the ego boundary and assume a false certainty, In schizophrenia external reality is confused with falsified ideas about reality, while the tendency of the inner boundaries of the ego to dissolve gives rise to feelings of depersonalization which are intensified by the recathexis of earlier and more infantile ego boundaries, so that there a multiplicity of ego forms cathected at the same time. In paranoia there is a falsification of thought sequences and the individual loses his capacity for logic and self-criticism. The character of planning explains the aggressive and systematic qualities of paranoid thinking. The manic patient continually abandons his pleasure-giving objects and ideas for new ones, as if they were sources of unpleasure.

In all these ideas (which are explained much more fully in the book and cannot be treated with justice in a few sentences) fascinating though they are, the ego psychology tends to leave us with momentous questions in our minds. Why do all these tremendously powerful ego-changes take place at all? What makes the ego withdraw its boundaries and regress to earlier states? Why are the ego boundaries sometimes invested with mortido? Why should the inner firmness of the ego dissolve and what is the nature of the powerful impulses and mental contents which then invade it? Why does the manic continually escape his love objects as if they displeased him? Why does the paranoiac substitute false steps of such vast moment in his thinking, and how can the quality of planning explain its savage but rationalized vindictiveness? If we turn to Melanie Klein we shall see to some extent how these and many other difficult questions might be answered.

There are many points in the book which might be taken up for discussion by a thoughtful reader, but no space can be found for them all here. For instance, it seems that the essential method of ego-therapy of the psychoses consists in helping the patient to come to insightful terms with his problems, and to re-repress the invading unconscious impulses and material, instead of lifting the barriers of repression as in treating the neuroses. It is therefore perhaps more of the nature of rehabilitation than of radical cure,

and possibly this is inevitable and may be one reason why group therapy is specially helpful to psychotics. A difficulty occurs early and runs throughout the whole book, in that Federn appears to think that the psychoses are partly determined in an essentially biological and hereditary way, but the problem of the interaction of heredity and environment in the causation of the psychoses is never fully discussed.

In spite of various criticisms which are not difficult to make, the book will excite much interest and deserves re-reading many times. It will introduce many readers to Federn's life-work which will provide a most valuable step forward in the understanding and treatment of the psychoses, together with partial and latent psychotic states and those essentially psychotic aspects of the neuroses which cause every analytic therapist particular difficulty when they are encountered. R. W. PICKFORD

*Brett's History of Psychology.* Edited and arranged by R. S. PETERS. London: George Allen and Unwin Ltd.; New York: The Macmillan Company. Abridged one-volume edition 1953. Pp. 742. 42*s*.

The re-issue of Brett's *History of Psychology* in an abridged (but still hefty) one volume edition raises some interesting questions about the different kinds of reader to whom the longer and the shorter versions may appeal.

Brett's original history, published in three volumes between 1912 and 1921, was a masterpiece of erudition, providing a survey of psychological theorizing from classical Greece to the end of the nineteenth century. He wrote as a psychologizing philosopher and often a very good one. The need to summarize made him sometimes a little sleep inducing. But he could be caught talking to himself in a way that was mature, humorous, insightful and often pithy. He rescued odd gems such as the probable first English use, in 1665, of the word 'anthropologie' in a book which divided the subject into psychology and anatomy. The writer announced that 'of the former we shall in a distracted rehearsal deliver our collections'. Brett clearly had a sneaking sympathy for his anonymous forerunner and one can understand why his students found him a stimulating and charming teacher.

Of his generation, and averse from talking down, Brett expected of his readers enough general background to pick up often cryptic allusions, without which equipment the significance of what he said was not always obvious. How much social and political history, for example, lies behind the remark that Descartes 'lived in close enough contact with beliefs about human souls taking up their abode in animal bodies to feel the immense advantage of a more scientific view of the matter'. There are stretches of the original Brett where the present reviewer is uncertain whether to criticize or to confess her own ignorance. Sometimes, however, the mid-twentieth century reader would wish to take Brett to task for not giving more of such history *en clair*, and showing its relevance to the cultural tradition in which modern psychology grew—and for that matter is still developing. Brett's medieval thinkers, for example, need the setting provided by an H. O. Taylor, a Huizinga or an Eileen Power. His Machiavelli and Pomponazzi droop a little unless the reader is prepared to meet them with some understanding of Italian Renaissance politics. Brett might be criticized, on the other hand, for having brought in too many minor deities, so that the wood is rather hard to see for the trees and there are details that could have been cut in the interests of someone wanting a broad outline.

Finally, and not irrelevantly, the original version, valuable reference book that it was, suffered because Brett was erratic in supplying immediate details of date and reference. Many of the latter he gave in notes at the end but his volumes were not handy for rapid consultation.

What have been the effects of Dr Peters' editing? First, we have now got one book instead of three and it is available instead of being out of print. Secondly, some of the sensible abridgement has helped to clarify the picture. Thirdly, by the cuts, new sections in clearly different type, new chapter headings and a final chapter on the twentieth century Dr Peters has given the book a rather different focus. The history as he produces it shows, more clearly than the original, changing emphases in the criteria of acceptable explanation and scientific method. To readers oppressed by Brett's philosophizing and detail, and barely convinced that the past of psychology has any relevance to understanding the present, these changes will be advantageous. Others may feel that the price of change of emphasis was too great a one to pay for the slant that might have been better achieved by the writing of a new book.

Dr Peters had a very difficult job. He shows he is well aware of the divergence between our own and Brett's approach when he comments that 'to many readers the revised edition will, perhaps, appear more like the Festival of Britain Skylon sticking out of the Crystal Palace than the winning design of Coventry Cathedral'. The present reviewer found the 1951 Skylon attractive and Dr Peters' counterpart is often useful and effective. Perhaps in a structure of his own making it could be a little more graceful and his

plea for recognizing the importance of adequate hypotheses might then be supported by his own suggestions of good ones to try out.

Dr Peters' cuts and section headings, justified as they often are, will have made it even more necessary for new readers to be armed with ordinary history. He has not rectified Brett's omissions of useful dates. His substitution, for Brett's notes, of a bibliography, in itself helpful, means that the reader who wants the original sources may still need to go to the older edition. The book could do with an index of subjects as well as names.

In sum: the new Brett is a different Brett, retaining a great deal (over 600 pages) of the original material, including many of Brett's most delectable irreverencies. It may attract an audience to which its predecessor will have been closed because appearing out of date and alien. It would be a pity if such readers, once tempted, did not then look up the older man, and with him do a little more exploring. It will be a great pity if Dr Peters confines himself to a piece of onerous editing and does not write the book of his own of which he is clearly capable. JOAN WYNN REEVES

*Cerebral Mechanisms in Behavior.* Edited by LLOYD A. JEFFRESS. London: Chapman and Hall Ltd; N.Y.: John Wiley and Sons, Inc. 1951. Pp. xi+311. 52*s.* net.

The Hixon Symposium, held at the California Institute of Technology in 1948, was distinguished by the eminence of its members, each in some particular approach to the problems of cerebral functioning. The six main papers in the published version have been revised by their authors; the discussion, which occupies nearly as much space, has been edited as little as possible—a policy which, although fairly certainly the best course, adds the difficulty of following a shifting debate to that inherent in the complex subject-matter.

The papers fall into two groups. The first, including papers by Von Neumann and McCulloch (one by Lorente de Nó could unfortunately not be published), argues deductively towards behaviour from hypothetical neural mechanisms, which are themselves derived from the elements of computing machines. The result, as several contributors point out, falls short of behaviour as empirically observed; and the theories proposed, though provocative, and perhaps capable of some degree of physiological confirmation, do not appear as yet to lead to hypotheses which might be verified in behaviour.

The procedure of the contributors in the second section is rather to argue back from observed behaviour to the mechanisms which might underlie it. Lashley considers the problem of the temporal integration of behaviour sequences in the light of current knowledge of reverberatory circuits; Klüver discusses functional differences between the occipital and temporal lobes as revealed by behaviour changes following extirpation; Köhler reports electrophysiological observations made in the course of his studies of figural after-effects; Halstead deals with the relationship between brain-functions and intelligence.

This presentation of a sample of the most advanced contemporary thinking in a difficult field leaves one with the feeling that we have not as yet sufficient understanding of either behaviour or cerebral mechanisms for one to throw any great amount of light on the other. This conclusion should not, however, be taken as a reflexion on the present volume, which clearly merits the attention of anyone interested in the development of physiological psychology. N. E. L.

(The Editor regrets the delay, for which the reviewer was not responsible, in the appearance of this notice.)

*An Introduction to Experimental Psychology.* By C. W. VALENTINE. Fifth edition. London: University Tutorial Press Ltd. 1953. Pp. viii+272. 8*s.* 6*d.* net.

The appearance of the fifth edition of this book, which was first published forty years ago, is testimony to its soundness and its acceptability to successive generations of students and instructors. It is organized in two sections—Part I now containing details of no fewer than fifty-seven different experiments (compared with twenty-seven in the original edition), Part II discussing the problems studied and the results obtained. The range is very comprehensive but with a bias towards educational applications. There is nothing, for instance, on emotion or on sensation or the psychophysical methods. There is nothing of brass-instrument psychology about it. All the necessary materials for the experiments are provided in the text or can be easily prepared. It is, avowedly, an elementary book. Only the simplest statistical treatment of results is called for. The experiments are simple but demand thorough and careful work. Any student who has worked through half of them, read the discussions and followed up the recommendations for further reading in a bibliography which has been brought right up to date, will have a really solid foundation for more advanced work and, almost certainly, a better understanding than one who has plodded through a textbook without doing any practical work. Prof. Valentine recommends, of

course, that his book should be used in conjunction with a textbook of general psychology. New experiments in the field of learning and perception and two corresponding chapters in Part II have been added in the new edition and the book now appears in a more modern format. The manual may be warmly recommended to those who are not already familiar with it and will continue to have a wide field of usefulness.

A. H. D. T.

*Uses and Abuses of Psychology.* By H. J. Eysenck. London: Penguin Books. 1953. Pp. 318. 2*s.* 6*d.*

This book comprises a non-technical account of the impact of applied psychology on human affairs. The author is vigorous in his defence of the subject as 'a science in its early formative period', candid in his readiness to say 'we don't know', and insistent that the right to criticize scientific work 'must not be assumed lightly' by the layman. Dr Eysenck is highly critical of those areas of psychology where research findings do not seem to justify present applications, though consistently constructive in his argument that indifferent work must, and ultimately will, be replaced by better. An interesting feature is the confidence shown towards the future development of a more rigorously scientific social psychology.

Psychologist readers may wish to take issue with Dr Eysenck on a number of matters, including quite fundamental ones. Those who regard science as concerned with a continuum of probability values may well dislike the author's frequent dichotomy as between 'no evidence' and 'adequate proof'. Although the book's emphasis upon the need for verification and falsification of alternative hypotheses in this field is to be welcomed, the treatment of psychoanalytic work seems unnecessarily severe. Nevertheless, few will wish to oppose reiteration of Freud's own sober advice against 'indiscriminate psycho-analysing'. Dr Eysenck is less than just to Freud whom he accuses of requiring that one 'believe in his system before one can criticize it', though Freud is doing little more than insist on adequate specialist training for psychoanalysts; Dr Eysenck's own standards of specialist training, for psychologists, are similarly exacting. Again, it is not always clear to the layman when the book is stating accepted as opposed to controversial conclusions. Many psychologists might agree, for instance, that the usefulness of psychotherapy has not as yet been scientifically proved, and that more research is needed. Without further evidence few would be willing to endorse certain other views expressed, for instance the tendency to explain predisposition to neurosis by genetic factors rather than genetic and early learning components together. Among hopeful signs of development is the readiness of Dr Eysenck, as a leading factorial investigator, to identify himself so closely with the standpoint of an experimental investigator of learning —in this instance Mowrer. A second encouraging sign is the author's similar sympathy towards psychoanalysts who *have* been very much concerned to verify their hypotheses (for instance the contributors to *The Authoritarian Personality*, one of the ten books recommended for further reading). Despite this, Dr Eysenck remains highly critical of psychoanalysis in general, his main objection being on methodological grounds.

In the interesting account of intelligence theory and testing the author is in close accord with the standpoint of Thurstone, but also reports some illuminating researches by Furneaux with some quite fundamental implications in this field. A convincing account is given of the handicaps which confront the psychologist, but not the physical scientist, in investigating a problem. Again, the Royal Commission which seeks and accepts mere opinions, whether specialist or otherwise, instead of conducting a scientific investigation, is heavily criticized. Finally, in political matters, Dr Eysenck strongly supports scientific inquiry as an independent alternative to doctrinaire attitudes of both the political Right and the political Left. The claims of a scientific psychology to play a larger part in human affairs have seldom been more ably defended. It is fortunate that this defence, as a *Penguin* publication, will be so widely read by non-psychologists.

PETER McKELLAR

*Psychomatic Approach to Gynecology and Obstetrics.* By Fritz Wengraf. Oxford: Blackwell Scientific Publications; U.S.A.: Charles C. Thomas, Springfield, Ill. 1953. Pp. xx+346. 50*s.* net.

This book is a monograph in The Bannerstone Division of American Lectures on Gynecology and Obstetrics, under the editorship of E. C. Hamblen. It is a valuable contribution to the subject, from psychoanalytic and psychosomatic points of view. It deals with the psychological and psychiatric aspects of all female generative functions, and is addressed primarily to physicians, gynaecologists and obstetricians. Apart from the value it has to them, it contains much material of general interest to all clinical psychologists. There is a bibliography of 516 works.

R. W. PICKFORD

*Introduction to the Psychology of Music.* By G. Révész. London: Longmans, Green and Co. 1953. Pp. xiv+261. 30*s.* net.

In Révész's own words, '. . . my main objective has been to introduce readers to the psychology of music—and at the same time give a systematic outline of my own research in this domain and that of acoustics.'

The book is divided into three parts: Part one deals with the physical attributes of musical tones; our musical system in terms of the different temperaments, with full lists of their ratios; the physiology of hearing; the theories of auditory perception and the physiology of voice. Part two describes the psychology of isolated tones and intervals outside the musical context, by the ear; the character of the different keys and the relationship between tone and colour. Part three is devoted to musical ability in its various aspects, including chapters on blind musicians, on the relationship between musical and other types of abilities, and on the inheritance of musical ability. Added are a few chapters only loosely connected with the psychology of music: on creative work; musical ability in the deaf; on the pathology of musical perception; the origin of music, and the relationship between aesthetics and music. The bibliography is restricted to books quoted in the text.

Taken as a whole, this book is a collection of articles on a wide range of subjects, some of which are only remotely connected with the psychology of music. Most of them vigorously polemic, they were written at different times, for a different purpose, and they vary in range, value and depth. Only by discussing each on its merits, could one do justice to them. The double object of writing an introduction to the psychology of music and of giving a 'systematic outline' of research extending over forty years, resisted the integration of heterogeneous material into a whole that could serve the purpose of both.

While stating that 'Music is not concerned with single notes and their relations as expressed in isolated intervals and chords', Révész devotes about one-third of the volume to these subjects. At the same time, he relegates the 'more complex tonal forms' to 'the realm of general music theory, aesthetics, or the laws of musical form and style'. The perception of music as gestalt, one of the fundamental problems of the psychology of music, is given little more than one page.

An introduction to the psychology of music should be a source of general information. It would, by its very nature, require an impartial and balanced presentation of the various aspects of the subject, complemented by a well selected and up-to-date bibliography as a guide for further information. For an introduction to the psychology of music this is a disappointing book. But the different chapters, each in its own right, make interesting and stimulating reading. MARTHA VIDOR

*Intelligence Testing and the Comprehensive School.* By B. Simon. London: Lawrence and Wishart Ltd. 1953. Pp. 112. 8*s.* 6*d.* net.

This is an intelligent and persuasive exposition of the familiar left-wing criticisms of intelligence tests and secondary school selection. It is worth the careful attention of psychologists, since many have in the past been too prone to believe that their tests do measure inborn and unalterable ability. But the author neglects to mention the more widely accepted current view that any hereditary potential depends for its realization on a suitable environment; that measured intelligence is therefore the product of genetic and environmental factors, and that comparisons of groups or individuals brought up in widely different environments (e.g. civilized against primitive) are illegitimate. Although this implies that the I.Q. is to some extent inconstant and affected by social class, intelligence tests do nevertheless possess useful reliability and validity as selection instruments; moreover, social class differences are themselves partly genetic. The irrefutable scientific evidence for these statements is ignored, since it would of course contradict the party line.

Mr Simon's complaint that the psychologist's concepts, such as intelligence, are conditioned by his cultural ideology, is also quite sound—in the light of contemporary field theory. Indeed there is much to be said for discarding the term 'intelligence' tests, especially at the 10–11 year examination, and substituting tests of 'educational aptitude'. But his account of the controversies over the definition of intelligence, and of the solution provided by factor analysis, is muddled and misleading. Far from the various definitions being 'mutually exclusive', it is because they—or at least the tests devised to measure them—overlap so much that it is possible to isolate *g*, and progressively to analyse out the verbal, spatial and other content factors and such formal factors as speed, facility with new-type items, etc., which often enter into our alleged tests of intelligence.

Much is made, too, of the circularity that is sometimes involved in arguments about the normal distribution of abilities; for the extreme socialist cannot stomach the idea that only a limited proportion of

the population has sufficiently high capacity to be able to tackle advanced secondary school courses. While it may well be true that present methods of streaming in primary and secondary schools do not give the more backward as good a chance to make up as more individual and flexible classroom organization, there was plenty of evidence of ineradicable intellectual differences in the days before intelligence tests or streaming in state schools had ever been thought of. And the claim that in Russia and Poland all children can be brought on at a relatively equal rate and achieve a much more equal level of educational attainment is so dubious that it can presumably never have been put to the test.

Many of the author's criticisms of the present system of secondary school organization and selection are justifiable. Its bad effects on primary school teaching are deplored by thoughtful educationists and psychologists. They would agree that there is no perfect method of selection for grammar schools, especially at such an early age as 10, and that numerous pupils relegated to modern schools (particularly in areas with low grammar school provision) might with a better education surpass some of those admitted. But to ascribe all these ills to the wicked psychologist and his class-conditioned concept of intelligence is ridiculous. Mr Simon nowhere attacks the parents whose class prejudices in favour of a grammar school education are largely responsible for the examination becoming so highly competitive. And he ignores the extremely complex historical factors and the political considerations which underly the present differentiation of our various types of secondary schools. It is probable that few of the people concerned with educational selection or guidance have so little influence on educational policy as the psychologist. Nevertheless, in the present writer's view, the time is ripe for psychologists to get together and attempt to decide how far their instruments are doing harm, and how they could be better employed. This is one reason why a rather slight book, which might be regarded by many as mere political propaganda, has been accorded a fairly lengthy review. Mr Simon has taken the trouble to study the technicalities of his subject far more thoroughly than most propagandists (though it is a pity that he relied so heavily on a book written by the reviewer 15 years ago); and his distortions of fact are remarkably few. He has made a serious case, and deserves a serious answer. P.E.V.

*Secondary School Entrance Examinations.* By A. F. Watts, D. A. Pidgeon and A. Yates. National Foundation for Educational Research Publication, no. 6. London: Newnes Educational Publishing Co. Ltd. 1952. Pp. 77. 3*s*. 6*d*. net.

This is the second interim report from the National Foundation on the allocation of primary school leavers to courses of secondary education. Its content is accurately indicated by the sub-title 'a study of some of the factors influencing scores in objective tests with particular reference to coaching and practice'. The authors survey a number of recent investigations into the effects of coaching and practice and describe some experiments of their own designed to answer certain definite questions about their influence. There remain some inconsistencies between the findings of different investigators which are not cleared up, and there are some puzzling results which are difficult to interpret. It is hard to see, for example, why both three hours and nine hours of coaching should consistently be more effective than six hours.

It is confirmed that the objective type of test is more reliable and fairer than any other kind of examination at the 11+ level, but it has not perfect reliability and is not always absolutely just in comparing pupils from different schools who have had varying amounts of preparation for the intelligence test. Further, it does not tell us all we need to know about the examinees and especially about the borderline cases. The authors disapprove of universal coaching for the intelligence test which has been recommended in some quarters and see no need for it. They advise sufficient practice to swamp the effects of differences in preparation—not a great deal is necessary. They have also a number of sensible comments and recommendations on the general problem of secondary school allocation and indicate where further research is especially needed. A.H.D.T.

*Shame and Guilt.* By Gerhart Piers and Milton B. Singer. U.S.A.: Charles C. Thomas. 1953. Pp. x+87. 23*s*. 6*d*. net.

This slim, elegantly-bound volume of 86 pages contains two essays, one by Dr Piers, the other by Dr Singer. They are concise and intelligible, and may well serve as models for the less restrained and more obscure of American writers on social psychology. The points made by each writer are quite straightforward. Dr Piers insists that shame and guilt represent different tensions, the former between the 'id' and the 'ego-ideal', the latter between the 'id' and the 'super-ego'. It may be that guilt masks shame,

as when a person is prompted by his sense of inadequacy to do something of which the super-ego disapproves, and then feels guilty; it may be that the reverse happens, as when a person seeks *shameful* punishment. This may make it difficult to detect the primary tension at work in any given syndrome, but the distinction is, nevertheless, of vital importance for adequate understanding and for therapy. The guilt-ridden and the shame-ridden have different personality structures.

Dr Singer, in his somewhat less conclusive essay, examines, in the light of what Dr Piers has said, the familiar distinction between guilt-ridden cultures, and shame-ridden ones. He shows that the distinction is by no means as absolute as it has appeared to those who wish to argue that the 'super-ego' is an invention of Western cultures. Having shown this, he unfortunately does not go on to discuss the causes of the differential emphasis (rather than clear-cut difference) which he seems to admit. However, his arguments against those cultural anthropologists who want to make a more extreme differentiation between cultures than the evidence warrants will be of great interest to all social psychologists, interested in alternative methods of socialization. W. J. H. S.

*The Deprived and the Privileged.* By B. M. Spinley. London: Routledge and Kegan Paul Ltd. 1953. Pp. vii + 208. 18*s*. net.

This is the book form of a Ph.D. thesis on a study of subcultures in Great Britain. The author, from New Zealand, attempts to give concrete form to the hypotheses put forward by Kardiner, Linton and others of a basic personality type and the existence of subcultures. It is assumed that if there are subcultures, very different typical personality structures will be apparent in each subculture as well as a basic personality type common to the culture group as a whole. Two very dissimilar subcultures are chosen and the method is to compare the development of children in the two groups, investigating whether different basic experiences make for different personality structures.

Dr Spinley's material for the study of the deprived group, a London slum area, makes fascinating reading for those engaged in any branch of social service work. She observes the personal psychological factors influencing the children—often being unwanted, lack of training in cleanliness, being quickly displaced in the mothers' care by another child, etc. It raises far-reaching practical questions of how best to deal with the delinquent and anti-social products of such an environment, by individual methods or by large-scale social medicine stressing different infant and child training and so aiming at altering the subculture personality pattern.

After the thorough treatment of the slum group, the study of the privileged or Public School group is disappointing and unconvincing. Dr Spinley is aware that much of her information on this group is second-hand and liable to distortion and merely hopes that in some areas of personality enough would be revealed to enable a comparison to be made. This section of the book shows clearly the difficulties and 'pitfalls' of culture studies. It is, for instance, assumed that the boys and girls attending Public Schools come from the same cultural group, but it is very doubtful whether many of the sisters of boys attending the best known Public Schools attend the rather different counterpart for girls. On the other hand, such important factors as being wanted, very early cleanliness training, the strong position of the father in the family, etc., do emerge.

The core of the investigation is a double comparison. From the data on early training, etc., she infers a personality pattern for each subculture group and then measures this against actual Rorschach records of adolescents representative of the groups. Secondly, the Rorschach records of the two groups are compared directly together. Briefly, the records bear out the subculture personalities inferred from the case material and the author tentatively points to certain trends which may belong to the British personality structure. Her conclusions about the latter are weakened by reason of the fact that a large percentage of the slum group is of Irish immigrant extraction and the relevance of an Irish culture seems to have been ignored.

Other criticisms of method might be made beyond the admitted shortcomings of the Public School group study but this is a pioneer work and both the omissions and its merits should be valuable in guiding and stimulating further experimental studies in this socio-psychological field. M. E. JONES

*Adolescence and Youth.* By Paul H. Landis. Second edition. New York, Toronto and London: McGraw-Hill. 1952. Pp. xii + 461. 42*s*. 6*d*.

The subject of this book is development between the ages of 12 and 24. (The period 12–16 is called adolescence, that from 16 to 24 youth.) The author is a sociologist and his thesis is that the adjustment problems of this period are posed mainly by the structure and climate of the society in which young people grow

up, and that the changes associated with puberty are not, by comparison, of much importance. He does not go so far as to say that adolescence is purely a social phenomenon but he is much more interested in the effect of environmental pressures which he discusses very thoroughly. The author finds that the situations are very different for those living in large cities, for those in small towns and again for those in the remoter country districts in which he is particularly interested. He gives a clear account of the most common problems which young Americans have to solve, grouped under the three main headings of attaining adult status in moral development, achieving 'marital adulthood', and becoming economically independent, showing how they differ in the different environments. The treatment of pubertal development and of the psychological reactions to the various social pressures is (notwithstanding the book's subtitle *The process of maturing*) brief and rather superficial. The book is well organized and well produced with numerous pie-diagrams, bar diagrams and other pictographic devices for presenting the social statistics, and the results of opinion surveys on which the author largely relies.

The chief interest of this work for British psychologists will lie in the obvious similarities and differences between growing up in Great Britain and in these three different American environments and the extent to which they can be correlated with differences in social structures and influences. A.H.D.T.

*Parent and Child.* By James H. S. Bossard. Philadelphia: University of Pennsylvania Press. 1953. Pp. x+310. 40*s*. net.

This book is not a comprehensive treatise on family life and relations between parents and children. Nor is it a contribution to knowledge as its subtitle 'Studies in Family Behavior' might imply: the studies are on the whole too slight for that. But if they are considered as illustrative of the text and as worked examples of suitable field exercises for the beginner, then the book will serve as a useful introduction to the sociology of the family. Its text is readily intelligible (the paragraph on pp. 105–6 is exceptional), it is interesting, especially its studies of the effect of family size, of remarriage, and of differences in class and age between parents upon family life and child development; and it prompts the reader to find out more for himself. H.E.O.J.

*The Psychology of Personality.* By Bernard Notcutt. London: Methuen and Co. Ltd. 1953. Pp. 259. 21*s*.

The author's preface to this book describes its purpose as that of 'direction finding in the field of Personality'. It is not a book about case histories, nor a technical manual of personality assessment, nor is it a résumé of research findings. The purpose is more to evaluate than to describe the activities of psychologists.

To achieve this purpose the late Prof. Notcutt begins by sorting theories about personality into three broad groups: those using some concept of basic traits (including factors or types), those describing a person in terms of environmental pressures, and those describing the interaction of person and environment. Under this last heading he devotes a chapter to theories of basic motives, which leads into one on patterns of maturation and another on the potentialities of psycho-dynamics. There follow three chapters entitled 'Projective methods', 'Men in groups', and 'The logic of validation' which are in effect brief considerations of methods of personality assessment. In the last seventeen pages Prof. Notcutt tried to outline historically the development of the concept of personality.

Prof. Notcutt has packed a great deal into 255 pages of short sentences remarkably lacking in jargon. Some of his antiseptic cutting and thrusting into the jungle of big trees, little trees, undergrowth and scrub which the psychology of personality resembles today is efficient. Students dazed by impending examinations may thankfully extract some of his tabular arrangements of apparently rival views.

But at this stage praise must give way to criticism. It seems likely that different psychologists would have wanted to take issue with Prof. Notcutt on different points. The reviewer would have wanted a more intelligible and more critical estimate of Lewin and to have asked Prof. Notcutt to go more fully into what he meant by spatial thinking. What he says suggests it is less of a virtue than he supposes. It is also hard to see for what readers the chapters covering personality assessment and history would be adequate. There is a second and more serious kind of criticism to be made. The book sets out to be evaluative, but in spite of its general lucidity it strikes the reviewer as so uneven in its insight, and to reflect such mixed attitudes to the material with which it deals, that one would hesitate to put it in the hands of a new reader without warning. Possibly only a student who is mature and critical enough to take the book lightly would be able to see how useful it might have been but for Prof. Notcutt's odd mixture of scathingly valid and of superficial comment. JOAN WYNN REEVES

*The Forgotten Language.* By Erich Fromm. London: Gollancz. 1952. Pp. 224. 16*s.* net.

*An Introduction to Jung's Psychology.* By Frieda Fordham. London: Penguin Books. 1953. Pp. 128. 2*s.* net.

*The Psychology and Psychotherapy of Otto Rank.* By Fay B. Karpf. New York: Philosophical Library. 1953. Pp. xii+129. $3.00.

*The Forgotten Language* is described as a first primer (for everyone) in dream interpretation, and as an introduction to the understanding of dreams, fairy tales and myths. It is based on the view that symbolic language is a language in its own right, in fact, the only universal language the human race has ever developed. As such, it is to be understood, says the author, rather than interpreted as if one were dealing with an artificially manufactured secret code. Dreams and myths are important communications from ourselves to ourselves, and we must understand the language in which they are written.

*An Introduction to Jung's Psychology* is an exposition for the general reader of the theories and technique of 'the foremost living medical psychologist'. It has an able explanatory introduction by Prof. C. A. Mace, a foreword by Dr C. G. Jung, and a biographical sketch of Jung. It is a clear and sympathetic exposition of Jung's psychology, and will be a useful addition to the books which are aimed at making his ideas intelligible.

*The Psychology and Psychotherapy of Otto Rank* gives an historical and comparative introduction to the theory and therapy of Rank. His relations to Freud, Jung and Adler and to significant developments in the fields of analysis, psychotherapy, counselling, education and social work, are explained. It contains the well-known photograph of the Freudian inner-circle, taken about 1922, as a frontispiece. Rank appears in this. There is also an interesting photograph of Rank taken in 1924 about the time of his separation from the circle after the publication of his book *The Trauma of Birth*, and another taken before his death in 1939. The present book was begun with Rank's co-operation, and has now been completed and published in response to many requests from interested persons. It will be valuable to have this account of Rank's personality, position in the psychoanalytic movement, and influence today.

R. W. PICKFORD

*The Psycho-Analysis of the Nursery.* By Alice Balint. London: Routledge and Kegan Paul Ltd. 1953. Pp. viii+149. 12*s.* 6*d.* net.

This book was published in Hungary in 1931 and subsequently translated into German, French and Spanish. But it is only now that it has been possible to produce an English translation.

It contains a clear and interesting statement of some of the most important contributions that psychoanalytic study has made to our fuller understanding of the emotional life of the young child. It describes in considerable detail the strains and problems which the child has to face in his efforts to come to terms with his environment, and the text is enriched and elucidated by examples of children's behaviour and conversation.

One could wish that this book had appeared in England some twenty years ago when the findings would have appeared less familiar and thus more thought provoking. To workers in the professional field it does not perhaps tell anything very new but it serves to reinforce and support our current views on child development. To educators and intelligent parents who already have a background of knowledge of child psychology it will prove stimulating reading, but it is not a book that will be read easily or without considerable hostility by the ordinary parent unaccustomed to such theoretical and often unpalatable concepts as the Oedipus and the Castration Complexes.

This book makes a strong plea for greater honesty of adults towards themselves and towards their children, for an effort to recollect what they knew as children but have in large measure repressed, and to allow the child to share some of the grown-up's feelings. It would be valuable if in addition a simple non-technical statement could be made in a subsequent publication showing more clearly how the knowledge gained from psychoanalysis can be applied in a positive way to the up-bringing of children. On reading this book one is left with the feeling that childhood is a very painful, frustrating and unhappy period when satisfactions are constantly postponed or renounced altogether. Insufficient emphasis is given to the relief from emotional conflict which the child obtains through play, through creative outlets, through acquirement of skills and gradually from new personal relations. It is clear that the translator has not been in close touch with modern developments in English education which allow greater expression

of instinctual energy and are less suppressive or authoritarian than would appear in middle class families in the thirties in Hungary.

The translation appears very satisfactory; the style is pleasant and the choice of words makes for clarity and conciseness. A.H.B.

*Developments in Psycho-Analysis.* By Melanie Klein, Paula Heimann, Susan Isaacs and edited by Joan Rivière. London: The Hogarth Press and The Institute of Psycho-Analysis. No. 43 in the International Psycho-Analytical Library, edited by Ernest Jones. 1952. Pp. viii+368. 30*s.* net.

*First Contributions to Psycho-Analysis.* By Sándor Ferenczi. London: The Hogarth Press and The Institute of Psycho-Analysis. No. 45 in the International Psycho-Analytical Library, edited by Ernest Jones (who was also the authorized translator of this book). 1953. Pp. 338. 30*s.* net.

The first of these books consists of a general introduction and a chapter on the genesis of psychical conflict in earliest infancy by Joan Rivière; followed by chapters on the nature and function of phantasy by Susan Isaacs; on certain functions of introjection and projection in early infancy by Paula Heimann; on regression by Paula Heimann and Susan Isaacs together; on theoretical conclusions about the emotional life of the infant, on observing the behaviour of young infants, on anxiety and guilt, and on some schizoid mechanisms, by Melanie Klein; and finally a chapter on the life and death instincts by Paula Heimann. There is a useful bibliography. An excellent index has been compiled by Judith Fay.

The book by Ferenczi was originally published in America under the title *Contributions to Psycho-Analysis* (later called *Sex in Psycho-Analysis*), and the present title has been chosen to avoid confusion with other collections of Ferenczi's essays. It is most satisfactory that this valuable book should be available now in the International Psycho-Analytical Library.

No space is available here for an adequate consideration of either of these books. Ferenczi's contributions to psychoanalysis are well known and have been duly estimated and appraised many years ago. The contributions of the school of Isaacs, Klein and others of the same line of thought, are still, unfortunately, in the reviewer's opinion, seriously underestimated in many psychoanalytic quarters, and the publication of this volume should help to clarify some issues, though it is unlikely to close many of the existing splits. It should be read and re-read by every person professionally interested in psychoanalysis and child psychology. R. W. PICKFORD

*The Threshold of the Abnormal. A Basic Survey of Psychopathology.* By Werner Wolff. London: Medical Publications Ltd. 1952. Pp. xv+473. 30*s.* net.

*Clinical Psychiatry for Practitioners and Students.* By Ian Skottowe. London: Eyre and Spottiswoode, on behalf of 'The Practitioner', and in 'The Practitioner' Series. 1953. Pp. x+395. 36*s.* net.

These are two useful books on psychopathology and psychiatry. *The Threshold of the Abnormal* is divided into six parts, which deal with the concept of abnormality, physiological, environmental and psychological factors of abnormality, treatment, and the diagnosis and evaluation of personality. The author starts by saying: 'While I am not a psychoanalyst of any creed, it is to the memory of Sigmund Freud, father of modern dynamic psychopathology, that the present survey is humbly dedicated.' There are in all eighteen chapters, each followed by a useful bibliography, and the book is embellished with a series of thirty illustrations, some of which are as fascinating as they are instructive.

*Clinical Psychiatry* is divided into two parts, the first dealing in five chapters with the principles of clinical psychiatry, and the second giving in fourteen chapters clinical descriptions of psychiatric disorders. There is a list of thirty-five 'key references' which is also intended to act as a guide for further reading. This includes psychiatric, psychoanalytic and psychological works. The principal aims of the book are to help the clinician to recognize and deal with psychiatric illness, to know how to examine the patient as a person, what to say to him and to his relatives, what should be done to help him and how to set about doing it.

Both books are to be warmly recommended as useful additions to the existing library of general works on psychiatry and psychopathology. R. W. PICKFORD

*The Psycho-Analysis of Artistic Vision and Hearing.* By A. Ehrenzweig. London: Routledge and Kegan Paul Ltd. 1953. Pp. xxxi+272. 25*s.* net.

This book is, of course, chiefly about vision and hearing as they operate in an aesthetic context. But its interest is greatly increased by the discussion of a number of aspects of everyday perception which are apt to be neglected because neither 'Stimulus'-theory nor Gestalt-theory throws much light upon their character and meaning. Such are the bizarre nature of films shown in reversed temporal direction, the role—constitutive not merely contingent—of roughnesses, transitive tones and other elements which do not appear in the score of a piece of music, and the striking changes in immediate visual appearance which occur as a fashion is born a disturbing novelty, achieves comfortable acceptance and eventually becomes assimilated into past history. Where aesthetic experience or intellectual appreciation are interwoven, the intractable features of perceptual processes become manifold. That official theory is impotent to deal with matters such as these is, in the view of Mr Ehrenzweig, no accident. For it is, at least in part, the function of such theories to conceal from conscious scrutiny the springs of much that moves us in our everyday perceptual experience. This inevitably leads the author into a difficulty—namely that coherent theory cannot provide the linguistic and conceptual tools for discussing what that theory is framed expressly to exclude from consideration. With this difficulty he struggles gallantly, but drags us with him into deep waters, where lurk strange monsters such as 'Gestalt-free structures'. An avowedly psychoanalytic basis assists him not a little in finding appropriate words and ideas. Where these fail he falls back, perhaps a little too readily, upon inverted commas. The book is not easy reading in consequence, but there can be no question of the wealth of interesting and important questions it raises and discusses. It is not easy to agree with all the author has to say. Not every biologically-minded reader, for instance, will share Mr Ehrenzweig's conviction (upon which he leans heavily) that a grave crisis in human history occurred when the adoption of the erect posture first concealed the female *genitalia* from view. Nevertheless, we must be grateful for his originality and courage in tackling so many problems in perception about which many psychologists adopt a defeatist attitude. R.C.O.

*Tragedy and the Paradox of the Fortunate Fall.* By Herbert Weisinger. London: Routledge and Kegan Paul Ltd. 1953. Pp. 300. 21*s.* net.

The author believes that the pleasure we take in tragedy comes from our responding to the fall of the hero as if it were equivalent to the death of the god or divine king in the myth of rebirth. This myth he believes to be an archetype, and therefore 'in responding to the concept of *felix culpa*, we are at the same time responding to all the layers of association embedded in the myth and ritual pattern in the long course of its development'. Unfortunately he offers no coherent, let alone cogent, argument for his belief. In place of this he traces, in a long, dragging catalogue, the development of ritual and belief centring in the death and rebirth of the divine king in the ancient Near East, in the Mediterranean area, and in Hebrew and Christian thought. There is a brief discussion of some modern theories of tragedy, a long and barely relevant attack on the Motion Picture Production Code and a disquisition on the concept of freedom for the arts and literature, with examples of prosecutions in the United States. Although the author's central theme might conceivably be argued more lucidly and convincingly, and to that extent is worth attention, the book as it stands is unprofitable. It is also disfigured by slovenly writing and spattered with such sentences as '...he sympathized with that attempt as only one who followed it in all its bitter and frustrating detail alone would'; '...it is quite clear that they did not, nor could they have had, any effect on the making of the Western mind'; 'suppose...that this assemblage then lays down the rules of the practice of these crafts, obedience to which the pilots and doctors are to be tested annually in open court'. And there are three mistakes in the few lines quoted from Milton on page 19. This degree of literacy (or care) falls short of what is needed for the writing of a book. D.W.H.

*Animals and Men.* By David Katz. London: Penguin Books. Revised edition. 1953. Pp. 192. 2*s.* net.

In the preface to the original edition of *Animals and Men* (1937), the author explains that he intends to present a 'few problems of animal psychology on which I worked with my students... (and) which contribute directly to comparative psychology'. This preface had been omitted in the new revised edition, and the reader is left with the unfortunate impression that this slim volume forms a representative survey of animal psychology.

Except for references to the Lorenz-Tinbergen school, little new material has been added. The reader will not find adequate reports of the intensive studies of subprimates at the Yerkes Laboratory or

Wisconsin, of the investigation of the behaviour of honey-bees by von Frisch, of the transplantation experiments of Weiss and Sperry, the work on orientation of lower animals previously reviewed by Frankel and Gunn (1940), nor of the innumerable experiments on the laboratory rat—special appetites, maze learning, brain mechanisms in instinctive and learned behaviour, etc.

Furthermore, some of the sections which should have undergone extensive revision have remained untouched. For instance, the discussion of homing and migration of birds makes no reference to the contributions of Griffin, Bissonette or Bullough, and the description of Koehler's insight studies, using chimpanzees, does not mention the follow up studies of Birch (1945).

Despite these omissions the book contains many interesting references to Katz's own extensive work in animal psychology, mainly with hens and monkeys, and surveys a considerable number of experiments and field observations by European workers up to the outbreak of the last war. Katz's general orientation is avowedly phenomenological; the method demands that each piece of research is undertaken with an unbiased description of the objects to be studied. Consequently, there are few excursions into theory, and except in the chapter on 'Needs, Drives and Instincts', theoretical problems are by-passed. We are handed certain unresolved puzzles, like homing and migration in birds, the homing of dogs, and counting in birds and monkeys, but are not taken behind the scenes, as it were.

The comparisons between animals and men are not always happy: 'there is a resemblance between the faulty development of wild animals in captivity and the neurotic's inadequate adaptation to his environment', or the refutation of the psychoanalytic view that thumb sucking is an expression of infantile sexuality by reference to Levy's experiments with puppies.

The present translation by H. Steinberg and A. Summerfield is excellent, and makes fluent reading. The book would have gained in its usefulness to psychologists had more of the original references been retained. However, it deserves to be widely read by those wanting a stimulating introduction to animal psychology. H.M.B.H.

*The New Way to Better Hearing.* By V. L. Browd. London: Faber and Faber. 1953. Pp. xii+252. 12*s.* 6*d.*

This book, by an American otologist, is addressed to two broad groups of readers: (1) those suffering from defective hearing, and (2) those concerned with their rehabilitation. It presents a non-technical account of the author's system of hearing re-education, together with chapters on the ear and hearing, and detailed proposals for a course of auditory training.

Dr Browd points out that in almost every case of deafness there is present some residual hearing capacity which is often not used by the patient. He bases his system upon training the ear to utilize this residual capacity to the full, particularly in the discrimination of speech sounds. Unlike some authorities he discounts specific training in lip-reading as 'preventing the freeing and conversion of hidden hearing power'. The detailed course of training requires the services of an untrained assistant, and takes up ten minutes a day.

The author's emphasis on perceptual processes in auditory training is stimulating and salutary, and much of his practical advice is sound. The book should be particularly valuable to a hard-of-hearing adult who is having difficulty in understanding speech or in using a hearing aid. One must remain sceptical about the value of the system with profoundly deaf patients: one frequently gains the impression from the book that in such cases useful hearing for speech can be restored. No experimental evidence is produced in support of this claim, although it is backed up by a number of anecdotal case histories. Dr Browd recognizes the need for social rehabilitation for the deaf, but his remedies may seem somewhat aggressive to English readers, and to smack of 'bookstall psychology'. In one of his demonstrations of hearing at increased distances he appears to ignore the fact that the inverse square law does not apply within an enclosed space of the kind he describes.

The book has neither index nor references. There are, however, eight excellent colour plates (very much better than the illustrations of the ear found in current psychological text-books) and its price is reasonable.

D. C. KENDALL

*A Guide to Mental Testing.* By Raymond B. Cattell. London: University of London Press. Third edition. 1953. Pp. xv+446. 35*s.* net.

This book was first published in 1936 and is too well known to require a detailed introduction. In conformity with the author's intention of revising, 'at intervals in response to the progress of research and the publication of new tests', a second edition appeared in 1948. The present enlarged and revised version contains a number of minor changes and a few major ones. Anachronisms which remained in the last

edition have been removed. For example, the reference to 'Cox's recent research' into tests of manual dexterity has been amended to read, 'Cox's basic research'. The chapter on the Measurement of Intelligence now contains new and more conveniently presented norms on the Cattell Intelligence Scales.

The major changes occur in the chapters on Tests of Temperament and Disposition, and Measures of Character Factors and Probes of Emotional Adjustment. In the first of these a Brief Selective-Answer Misperceptive Test of Personality Factors has been included. It has been constructed to measure, 'five of the more important and firmly established primary personality factors'. Two other new tests of personality have also been included. One is a humour test and the other a musical preference test. Both are reported to distinguish normal from abnormal populations.

In the second of the chapters mentioned above Cattell discusses his Sixteen Personality Factor Questionnaire and includes the items from the shortened form of this test. This is apparently intended to replace the Questionnaire for Personality Factors which was published as Appendix II in the last edition.

Among the tests that users of this guide might wish to see mentioned is likely to be the Wechsler Intelligence Scale for Children. Although this test is becoming increasingly popular no reference is made to it. It is perhaps unfortunate that the theoretical limitations that the author imposes upon himself are passed on to the practising psychologist in this way. However, if these limitations are accepted, *A Guide to Mental Testing* provides a convenient reference to a wide range of useful tests. A. RICHARDSON

*The Fourth Mental Measurements Yearbook.* Edited by O. K. Buros. Highland Park, New Jersey: Gryphon Press. 1953. Pp. xxvi+1163. $18.00.

This reference work is designed to help test users to 'locate and evaluate tests and books on testing' (p. xix). It covers the period 1948–1950 and thus complements rather than replaces its predecessor *The Third Mental Measurements Yearbook* (covering 1940–1947). Each test is named and numbered, information is given about the source of supply and cost on the one hand, and about standardization, reliability and validity on the other. This is followed by evaluative reviews written specially for the *Yearbook*, together with excerpts from reviews published elsewhere. Comprehensive lists of book and journal references to the test are also included. The main part of the book (830 pages) is devoted to these test entries, the tests being classified on the system used in the earlier *Yearbook* with only minor modifications. The remainder of the book deals with reviews of books relating to mental testing, directories of periodicals and publishers, and three indices: names, titles and tests.

Dr Buros has again provided psychologists, educationalists and other test users with a most valuable reference book. He has, moreover, performed an additional service that becomes apparent immediately one starts to use this book or its predecessor. In the detailed instructions given to the contributing test reviewers Dr Buros states his aim: 'to stimulate progress towards higher professional standards in the construction and use of tests, by commending good work, by censuring poor work, and by suggesting improvements'. This editorial policy, supported by more than three hundred special reviewers, makes the book valuable not only as a work of reference but also as a very thorough piece of 'reality testing' of one of psychology's most important tools. Good, well-standardized tests are given the credit due to them, but no reader can long remain unaware of just how many tests of unknown reliability or uninvestigated validity are in widespread use. The reviewers have not failed in the task assigned to them of making their criticisms specific, of mentioning alternative better-standardized tests when available, and of indicating weaknesses or omissions of fundamental kinds in test construction and standardization. The book is, as a whole, rather depressing in that it indicates just how widespread faults of this kind are; it also serves to make this fact unmistakably plain to the world of professional psychologists. Those who consult it must inevitably come to sympathize with its editor and his contributors in their efforts 'to impel test authors and publishers to present more detailed information on the construction, validation, uses and misuses of their tests'.

These books, which began with *Educational and Personality Tests of* 1933 *and* 1934, have shown progressive development. Future volumes (which it is hoped to publish every three years) might perhaps develop a stage further to include a separate list of approved, properly standardized tests. The residue of the book could comprise tests of dubious value, together with those at the quite legitimate stages of research development; this residue would form the main body of the book. It is unfortunate that both this and the earlier *Yearbook* are so bulky; this limits their usefulness, since it is a laborious task for each reader to evaluate the better tests for himself. A place on such a 'white list'—one hesitates to suggest a 'black list'—might in the course of time come to be eagerly sought. This would greatly hasten the achievement of the wholly admirable aim of the book's editor and contributors. Separate publication of

the section on books as opposed to tests would also diminish size and costs. Again the present system of test classification is not wholly satisfactory; larger printed numbers would assist quicker location of test entries; the Universal Decimal System of numbering merits consideration; and tabulation might be used to shorten the test reviews. Yet it would be ungrateful to press minor criticisms of a major contribution towards the more rigorously scientific developments of psychology. Despite its cost this is a book which no responsible institution concerned with psychological testing can afford to do without.

PETER McKELLAR

*The Tools of Social Science.* By JOHN MADGE. London: Longmans, Green and Co. 1953. Pp. x+308. 25*s.* net.

Methods grow out of problems and a periodic evaluation of the techniques for the collection and analysis of data is a necessary part of any developing science. This evaluation forms the main section of Madge's book and provides at the same time a well-presented introduction to such topics as the use of documents, observation and interviewing methods, attitude scaling, sampling procedures and the design of experiments in social science.

However, this book is more than its title might suggest, since about a quarter of the total space has been devoted to a discussion of such fundamental theoretical and practical issues as the place of value judgements, the nature of objectivity and the function of language and logic. For example, in discussing value judgements the author writes: 'Instead of aiming to become the objective value free theorist, the social scientist may without shame take on the job of social engineer, in which he may use his experience and special skills in the service of his chosen community.' This is a recurrent theme throughout the book and one which is integrated with the evaluation of the techniques at present available.

'Exactness is beyond our reach because of the penumbra that surrounds all our observations, and universal truth is not possible because every truth that we adhere to contains within itself some part of our aims, and aims are never unanimous....The truth that is powerful in the world is the truth that is sufficiently exact to get things done.' These views may not commend themselves to the purists but, in a period when social science has tended to retreat into the abstractions of theory and technique, they supply a timely counteractive.

A useful source of relevant references is provided by the footnotes which are neither too intimidating nor too distracting. As a whole the book is well presented, the elimination of much technical jargon making it as suitable for the interested layman as for the social science student. A. RICHARDSON

*Appraising Personality.* By MOLLY HARROWER. London: Routledge and Kegan Paul Ltd. 1953. Pp. xviii+197. 18*s.* net.

This is an attempt to make known to doctors, psychologists and a wider public something of what takes place when the clinical psychologist assesses personality by projective methods. It is in the form of dialogues between a psychologist and a physician, the latter a patient pupil and a little of a Watson—a way of presentation that proves somewhat trying. However, the exposition is clear, simple and free from jargon. The projective techniques include the Rorschach, drawing of the human figure and of the 'most unpleasant concept', the Szondi, and a promising sentence-completion test. Personality clues elicited by the Wechsler Bellevue test are also used. No attempt is made to cover the techniques of administration, the aim being rather to illustrate the information that can be obtained and the use that can be made of it.

There is a suggestion that the projective responses are regularly striking and impressive, the psychologist always right and the outcome invariably successful: this is perhaps but the result of the attempt to write a popular book on a notably tricky subject. Something of the difficulties and limitations as well as the conspicuous successes of the techniques would have given a more realistic picture and, probably, added to the reader's confidence in the methods discussed. However, the author's handling of the projective material is shrewd and clearly rests on sound experience.

*The Yearbook of Psychoanalysis.* Vol. VIII. Edited by SANDOR LORAND. New York: International Universities Press. 1953. Pp. 383. $7.50.

This volume of *The Yearbook of Psychoanalysis* collects re-printed contributions by twenty-five authors, including Freud himself and many well-known writers on psychoanalysis and related subjects. It is full of interest for the technical reader and many of its contents will command the attention of the general psychological reader as well. It maintains the high standard and general interest of previous volumes and will be a stimulating book for addition to any library containing works of psychoanalysis.

R. W. PICKFORD

*Proceedings of the Fourth International Congress on Mental Health, Mexico City, December, 1951.* Mexico: La Prensa Medica Mexicana. London: H. K. Lewis and Co. Ltd. New York: Columbia University Press. 1952. Pp. xvi+386. Eight photographs of the Congress. Four photographs of murals by Guillermo Meza. 30*s.* net.

To all psychiatrists, psychotherapists, psychologists, social workers and others professionally interested in mental health, this volume of the proceedings of the Fourth International Mental Health Congress will be of the keenest interest. It is edited by Alfonso Millan, and contains, in English, a complete record of the work of the Congress, which covers every aspect of the problems of mental health, psychiatric, psychological, psychoanalytic, sociological, educational and racial. R. W. PICKFORD

*Catalogue of Films on Psychology 1953.* London: The Scientific Film Association. 1953. Pp. vii+118. 7*s.* 6*d.*

This is an extremely valuable publication. As a catalogue it is not quite complete. There are, for instance, a number of films on animal learning and child development held by the University of St Andrews which are not included; but this is a defect which will easily be remedied now that a first step has been made, since all those departments who have films will no doubt make a point of having them listed.

What gives this catalogue its value is the short appraisal given for each film. The reviewer has checked the appraisals of those films he has seen and finds himself in complete agreement with them.

It is perhaps unfortunate that so many of the films that have been made in the field of psychology are of a rather superficial kind and directed towards a general audience. It may be that the expenses involved make the production of more technical films difficult, or it may be that the film itself is not a suitable vehicle for the conveying of complex information. This does not, however, detract from the value of the job done by those who have compiled this catalogue which should be in the hands of everyone running courses in psychology, education or social sciences. JAMES DREVER

## CORRIGENDUM

It is regretted that the author of *The Ames Demonstration in Perception* reviewed in Vol. XLIV, p. 268, was given incorrectly. It should be William H. Ittelson.